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Photo by R. E. Stinson

Vulcan's Face—A peculiar smoke cloud seen during the eruption

THE ERUPTION OF LASSEN PEAK—[See page 216]

Relativity and Gravitation*

A Discussion of Attempts to Bring Gravitation Within the Scope of the Principles of Relativity

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THE first of the two organized discussions arranged for the Mathematics and Physics Section of the British Association was devoted to the subject of Gravitation.

A considerable part of the discussion was occupied with the recent work of Einstein and Grossmann which attempts to bring gravitation within the scope of the principle of relativity. This latter principle, now widely accepted, denies the possibility of determining *absolute* motion in space by means of any physical phenomenon. The various attempts to determine the velocity of the earth relative to an assumed immovable ether and the consistent failures of these experiments to detect such relative motion either by optical or electrical methods, have accumulated a considerable body of evidence in favor of the principle.

In Einstein's earliest formulation of the principle of relativity (*Ann. der Physik*, xvii, 913, 1905) he stated it in these words:—The laws of nature are independent of the state of motion of the system of reference provided this is unaccelerated. This means in more particular terms, that if two sets of observers were stationed in two different worlds, and each set made ideally careful and precise measurements on all natural phenomena, then they would each summarize their observations in precisely the same laws, provided each of the worlds was moving *uniformly* with respect to the other. So it would appear that natural phenomena would fail to decide as to which of the two worlds was absolutely at rest in space and which in motion. Such phenomena could only yield a measure of their relative motion. Indeed, as far as purely mechanical phenomena are concerned the principle is as old as Galileo, and is employed by every student who works out a problem in Rigid Dynamics involving the use of "Moving Axes." It was the continued failure of optical phenomena to reveal any difference in the measured velocity of light, whether the light was traveling in the direction of the Earth's motion, or against it, or across it, which led Einstein to extend the principle so as to include all phenomena, optical and electromagnetic, and in particular to enunciate the special case of relativity known as the principle of the constancy of the velocity of light, viz., that the velocities of light measured with respect to two different systems of reference will be the same if the two systems are in *uniform*, relative motion one to the other. It follows from this postulate that in transforming from a set of axes fixed in one system of reference to a set of axes fixed in the other, not only are the coördinates of a point with respect to the one set linear functions of the coördinates referred to the other set and the time (which is to be expected), but the time of the occurrence of an event at this point in the first system of reference is also a linear function of the coördinates of the point in the second system and the time of the event in the second system. It is this second fact which constitutes such a novel departure from our ordinary notions as regards the invariability of the time measure of an occurrence. Further it appears that even the linear relations connecting the coördinates in one set with the coördinates and time in the second set, are not those derived from the usual "principle of moving axes" as employed in works on Dynamics. In fact these relations are so modified that they have precisely the same form, whether one in transforming from the first set of axes to the second, or from the second to the first, and thus, as stated above, it is impossible by means of such relations to determine which of the two systems of reference (if any) is at absolute rest; they merely indicate a relative motion of one system to the other with a velocity uniform in amount and direction. The existence of these relations leads to a number of results which seem paradoxical at first to our preconceived ideas concerning space and time; thus it appears that when a body originally at rest *in a definite system of reference* is set in motion, its dimensions measured relative to axes in this system are contracted in the direction of motion and unchanged in planes perpendicular to this; its dimensions relative to a system of reference moving with it are the same as they were originally. But this result is precisely the assumption which Lorentz and Fitzgerald introduced a few decades ago to account for Michelson's failure to detect the (assumed) influence of the Earth's motion on the velocity of light. A further deduction from these relations concerns the time registered by a clock between two definite occurrences. If this interval be measured by two clocks each one fixed in one of two systems of reference in relative motion, the two observations will not agree; in fact, if the pendulum of one of the clocks could be observed from the other system, it would appear to have its period increased beyond the value it has when measured by observers at rest relative to it. The effects referred to depend on the square of the ratio of the relative velocity of the two systems to the velocity of light, and it appears hopeless at present to obtain (say) two platforms moving on the Earth's surface with a relative velocity large enough to produce these anticipated results in large enough magnitude to be measurable. Einstein's views and the four-dimensional interpretation given to them somewhat later by Minkowski present the concepts of space and time not as two distinct modes of perception, but as two closely related aspects of reality. Very lucid and sound accounts of the principle may be found in Campbell's "Modern Electric Theory" (2d edition) and Richardson's "Electron Theory of Matter." A very complete exposition is given in Cunningham's "Principle of Relativity.

The work with which the British Association meeting was immediately concerned was the recent attempt of Einstein to include gravitational phenomena within the scope of a "Generalized Relativity Theory." The inclusion of gravitation within any of the recognized electric theories has always presented considerable difficulties, based in all probability on the fact that the known laws of gravitational action are so exceedingly simple, paradoxical as that statement may appear. As the subject left Newton's hands, no question was raised as to the possibility of the gravitational action of one body on another being an influence which requires time for its propagation from the one body to the other. With the advance of elastic solid and electromagnetic theories of the ether, such views as to the propagation of gravitation through space with a finite velocity naturally came forward, and when Einstein's relativity theory began to be accepted with its necessary restriction that nothing could travel faster than light, the question was seriously raised as to the possibility of reconciling astronomical phenomena with the view that gravitational action was propagated through space with the velocity of light, a view which would accord with the theory that gravitation is the result of an "uncompensated residue of electrical forces" (to quote Prof. Richardson). Now Lorentz has considered this question very fully, and finds that if gravitational action is propagated in the same manner as electrical actions, it will give rise to effects practically identical with those which follow from the usual Newtonian law. The differences are too small to be detected, and they are incapable of accounting for certain recognized discrepancies in the motions of the heavenly bodies. Relativity theory would therefore treat Newton's theory as a first approximation, but without any new experimental indications as to the direction in which an extension is to be sought; as mentioned, the simplicity of the known gravitational laws permit, as it were, too much freedom; there is but little restriction on the number of possible assumptions and insufficient definition of the direction in which progress is to be made.

In his "Generalized Relativity Theory" Einstein makes an attempt to develop gravitational theory in a manner consistent with known facts, so as to bring it within the scope of electromagnetic actions; in doing so he has scored a notable success in removing one of the most serious discrepancies known between astronomical theory and fact. In the earlier relativity theory Einstein assumed the equivalence of two systems in uniform relative motion as regards the summarising of phenomena in laws. In the new development he assumes the equivalence of a homogeneous field of gravitation and a system of reference with uniform *acceleration*. The former is a region where the observer is at rest, relative to a stellar body whose attraction causes all free bodies to move towards it with an acceleration uniform throughout the region considered. The latter is a system in which the observer is at rest, *but freed from the attraction of the star*, system and observer moving with a *uniformly accelerated velocity* relative to the star. Einstein's hypothesis is that these two systems are physically identical, and that it will be impossible to distinguish between them by the formulation of the laws concerning phenomena as observed from each system. This amounts in fact to a denial of the possibility of detecting absolute acceleration, just as the relativity principle denies the possibility of detecting absolute velocity. The equivalence of the two systems is obvious enough, so long as we confine ourselves to Newtonian Dynamics, just as a limited case of relativity was in constant use even from the time of Galileo. Thus, to take the illustration which has been frequently pressed into service to assist the beginner in grasping Einstein's idea, consider an observer situated in a closed lift, observer and lift being free from the attraction of any gravitating matter. If the lift were in uniform motion, there would be no pressure between the lift floor and the observer's feet, in fact by a gentle spring the observer could "float up" to the ceiling of the lift; bodies placed anywhere in the lift would remain there without support —in short there would be no "up" or down." If now the motion of the lift was accelerated all the mechanical phenomena which we associate with a field of gravitation would supervene. If the acceleration is maintained in a direction which is "upwards" (in the sense from feet to head), the lift floor would exert a pressure on the observer's feet in proportion to the magnitude of the acceleration; bodies would "fall" (i. e. move towards the lift floor) with an acceleration *relative to the lift* equal in magnitude and opposite in direction to the acceleration of the lift. Now Einstein's hypothesis amounts to this; suppose the observer can observe phenomena through windows in his lift and make precise measurements, then all events, optical, electro-magnetic, etc., as well as mechanical, will follow the same course for him as they do to the observer stationed on the gravitating star. The first deduction from this is of extreme interest, for to the observer in the accelerated lift a ray of light (which would be straight to an observer in a uniformly moving system) would appear curved, the deflection from the straight path being opposite to the direction in which the lift is accelerated. If, as Einstein supposes, this will also be the appearance to the observer fixed on the gravitating star, then we reach the important conclusion that the passage of a ray of light near to a gravitating body should produce a bending of the ray toward the body. The curvature to be expected is extremely small—amounting to a change of direction of 1.7 inches in the case of a star seen close to the sun's limb—and it is impossible so far to prove or disprove the hypothesis directly. However, Einstein has succeeded by the development of the idea, thus roughly sketched, in throwing the laws of motion, of electrodynamics, and of gravitation into a form which makes the sequence of phenomena entirely independent of any particular framework of reference, and he has been able to predict therefrom a motion of the perihelion of Mercury amounting to 43 inches per century—just the amount of a motion which has hitherto been regarded as a serious discrepancy between fact and the usual Newtonian theory. Those who are interested will find an elementary account of the matter in Chapter XXII. of Richardson's recent book "The Electron Theory of Matter." For those who can follow the mathematical development of the relativity principle, an explanatory paper by Dr. Fokker in the "Phil. Mag.," January 1915, will prove useful; full references to the original literature are given in this paper. Another remarkable consequence from Einstein's theory is pointed out by Fokker in the paper referred to. It was remarked by March in his "Principles of Mechanics" that we cannot speak of mass in an absolute sense—mass is a relative measure of the inertiæ of bodies, determined in the case of two bodies by their relative accelerations at an instant when they are mutually acting on each other, and free from other influences. This should not be confused with the idea of gravitating mass, the measure of which makes its appearance in the usual Newtonian law of gravitation. March concluded that we are not justified in thinking of the mass as of something absolute belonging to a body, but that it may be due to some inducing influence of bodies one upon the other. If this be so we should expect to find that the inertial mass of a particle is increased by the heaping up of other particles in its neighborhood, and that an accelerated body induces an acceleration of the same direction in other bodies. These early speculations of March follow as direct conclusions from Einstein's theory.

At the same section, Dr. P. E. Shaw gave an account of his experiments, which appear to indicate a change in the gravitation constant with the temperature of gravitating bodies. Dr. Shaw has already published a statement of his work in the "Phil. Trans." A. 216 (1916).

*From *Science Progress*.

The experimental work has been of extreme delicacy and has, it would appear, been going on for close on ten years, some earlier types of apparatus having proved of no avail in the research and having to be abandoned. The apparatus ultimately used was of the Cavendish torsion-balance type in the form adopted by Boys in his researches on the value of the gravitational constant. The suspended system was enclosed in a vacuum vessel in which the pressure of the residual air was varied from about 1 micrometer of mercury for some experiments to as high as 15 millimeters for others. The large masses of lead whose gravitating effect on the suspended system was being observed had their temperatures altered by electric heating between limits 15° C. and 250° C., the suspended system being maintained at ordinary laboratory temperatures. The utmost precautions were taken to guard against spurious effects due to electrostatic or magnetic action, or to convection in the residual air or its radiometric pressure, or even to the radiaton pressure of the lamp used to illuminate the mirror on the suspended system. In the first instance it would appear desirable to test if the gravitation constant varied according to some expression linear in the mean temperature of the attracting masses (each temperature being "weighted" by the corresponding mass in obtaining this mean). On account of the large mass of the lead spheres or cylinders in comparison with that of the suspended system, this mean temperature would be practically that of the lead bodies. Dr. Shaw is unable to assert from his experiments that such a linear relation does hold, but he states that he has been able "(*a*) to obtain consistent cyclic readings in a gravitational experiment of the Cavendish type, even though the large masses are maintained for hours above 200° C., while small masses remain at ordinary temperature; (*b*) to carry on this investigation in the center of a city at any time by day or night, in spite of the attendant tremors and the special disadvantage of having the torsion balance in a vacuum;" and further to conclude that there is a temperature effect of gravitation. "When one large mass attracts a small one, the gravitative force between them increases by about 1/500 as the temperature of the large mass rises from, say, 15° C. to 215° C. At present the result is provisionally stated as being $+1.2\times10^{-5}$ per centigrade degree; but the readings are not steady enough to justify the statement that there is a *linear* relation connecting the gravitation constant and temperature."

Dr. Shaw, in a short review of the previous work on the constant of gravitation, points out that, though much of it yields no evidence one way or the other on this point, yet a part of it does lend some small weight to the view that there is a positive temperature effect for gravitational attraction.

Cast Iron Growth

THOUGH cast iron is sometimes used to symbolize immutability and inflexibility, it is a fact well recognized by engineers that actually this material is often far from stable. It possesses the peculiar property of "growing" under certain thermal conditions. This property makes cast iron a decidedly unsuitable material with which to construct many parts used in engineering work that are subjected to repeated heating. Examples are on record where there have occurred growths of upwards of 40 per cent. Though the latter cases are rather extreme ones, a case is known where a cast iron steam turbine casing had grown 7 per cent, which was quite enough to destroy its efficiency. Another was of a high-pressure steam valve which had cracked, and swollen badly in service, and was rendered useless. The subject, to which scientific attention has only recently been paid on the engineering side, is one, obviously, of very great practical importance. It was selected by Professor H. C. H. Carpenter, a member of the Advisory Panel of the Admiralty Board of Invention and Research, as the basis as a paper which he presented at Dudley to the South Staffordshire Iron and Steel Institute at a recent meeting.

After reviewing in the light of recent experience the earlier investigations made on this subject by Professor Rugan and himself, Professor Carpenter summarized the position in the following words:

> The chief sinner in the swelling of grey irons on heating, whether at high or low temperatures, in air or furnace gases or superheated steam, is unquestionably silicon, present, not as such, but as dissolved iron silicide, in the main bulk of the iron. But this silicide can exert no influence whatever in producing growth by separation as silica and iron oxide unless graphite is present, which on heating the iron allows oxidizing gases to penetrate into and gradually attack the metal chemically. Without graphite silicon is absolutely innocuous.

Professor Carpenter's work leads us, undoubtedly, to the conclusion that if any grey iron is to be heated in use, either repeatedly or for prolonged periods, either in superheated steam above 250 deg. C., or in air or furnace gases above 650 deg. C., then it is useless to expect it not to swell and eventually to crack. The growth would be expected to be least for the cold-blast Staffordshire low silicon irons, and most for the high silicon hot-blast irons. As regards the former, Professor Carpenter said that he anticipated that growths of the order of 10 per cent would occur, while in the latter the figure might easily be 40 per cent. This being the case, it is clear that for any of the conditions mentioned grey cast iron is an absolutely unsuitable material if it is essential that it should not grow. Many of the white irons, also, would have to be ruled out, seeing that the solution of the growth difficulty is only to be found by using a material which, in addition to not containing any free carbon, does not deposit it on repeated or prolonged heatings. As the majority of the white irons, however, suffer from the disability of brittleness, as a result of which they almost invariably crack on heating, they are not of special importance in this connection.

No grey cast iron, and, with very few exceptions, no white irons, being really suitable to withstand repeated heating, Professor Carpenter discussed possible alternatives, and pointed out that there exists a comparatively wide choice of suitable materials, amongst them being mild, medium, hard, and very hard steels, that are now placed on the market. His investigations had shown that over the entire range covered by these steels, namely, from 0 to about 2.5 per cent of carbon, there would be at most only a slight growth or shrinkage on repeated heatings. The reason for this is clear. There is no free carbon in any of them; none is deposited on heating, or, at any rate, only under exceptional circumstances, and in no case is the silicon high—the conditions of swelling are, therefore, absent.

Beginning with the lowest temperature at which the growth phenomenon becomes prominent, Professor Carpenter took as an example the case of high-pressure steam valves. Here the material has to stand not only prolonged or repeated heating under a steady pressure, but also shocks due to water-hammer and expansion, and pipe strains. It is clear, therefore, that a metal, not only of adequate tensile strength, but also of considerable ductility, is required. These conditions can be sufficiently met, in the opinion of Professor Carpenter, by the use of good quality open-hearth mild steel, the case being mentioned of a firm of engineers which has now, as a result of Professor Carpenter's investigations, entirely discarded cast iron in favor of cast steel, and has built a foundry specially for making valve bodies and other parts of cast steel.

In steam turbines, where high temperatures are also encountered, cast steel is steadily replacing cast iron. An instance of this has been given by Mr. A. Coulston, whose works had in for repairs some large reaction turbines that had been running with high steam superheats, and in which the high pressure stages of the turbines had grown so much that the clearance of the blades had entirely gone, and the casing was much distorted. The repairs consisted in cutting off the early stages of expansion and fitting entirely new ends made of steel, so that the makers have now adopted this as their standard practice in the manufacture of high pressure turbines using superheated steam.

Proceeding up the temperature scale, Professor Carpenter said that hot blast valves furnished a very difficult case, these valves being subject to alternate heating to 700 to 800 deg. C., and cooling to 200 deg. C., their growth being very considerable. Having regard to these temperatures, and to the fact that the gas to which the valves are exposed is very oxidizing in character, he was doubtful whether steel could here be recommended in place of cast iron. He thought that probably the best that could be done was to use a very close, fine-grained cast iron with as low a silicon and as high a manganese content as could be secured.

Though cast steel could not be recommended in this case, Professor Carpenter advised its employment in the manufacture of annealing pots, which when made of cast steel, had a life three times that of the cast iron pot.

The question of a suitable material becomes much more difficult when it has to withstand temperatures of well over 1,000 degrees as an instance of which we have the grate bars of the mechanical stoker, which are usually made of high-grade foundry iron. After discussing the difficulty of providing suitable material for these bars, and also referring to the cracking of slag ladles—less harmful in the case of cast steel than in the case of cast iron ladles—Professor Carpenter turned to the important case of those parts of internal combustion engines which are exposed directly to burning gases, in which case difficulties arise from what is regarded as the growth of cast iron under working conditions. In these engines a mixture of gas and air is compressed, compression being followed by combustion which creates a very high temperature and a considerably increased pressure, as well as highly oxidizing atmosphere. The cycles succeed one another in rapid succession, so that the exposed engine parts have to withstand very severe conditions indeed. In the Diesel engine, on the other hand, there is high initial compression, but only of air, so that the degree of compression is not limited by the temperature of combustion of the mixture in the cylinder as in the case of gas engines, where there is risk of pre-ignition, and in consequence a high temperature is attained towards the end of the compression.

As extremes reached in a Diesel cylinder, Professor Carpenter mentioned the temperature of 1,600 deg. C. and 210 deg. C., there being subject to these temperatures the cylinder head, the cyclinder, including the liner, and the piston, all usually made of cast iron though cast steel is occasionally used for the cylinder head.

According to Professor Carpenter, the "seizing" of pistons in certain of these engines has been traced to the permanent swelling of the cast iron, and in this connection a well-known engineer wrote to the professor as follows:

> I believe your former paper helped me to get over an apparently hopeless difficulty, namely the cracking of some large Diesel engine pistons. I suggested that it might be due to stresses set up by "growth" and that it might be got over by steel heads. I am told that this was done, and so far as I know there have been no further cracks.

In this connection, however, it would appear that Professor Carpenter is not quite satisfied that the conditions prevailing can be entirely correlated to those under which his own "growth" experiments were carried out, and he quoted Mr. J. E. Hurst's recent views, which are to the effect that "growth," in the strict sense of the term, is not met with in the Diesel engine, and that the cracking phenomenon is due (1) to the alternating solution and precipitation of free carbon in the cast iron which is aided by the presence of phosphorus; (2) to the liquation of the phosphide eutectic; and (3) to the alternating variations of the internal gas pressure. Professor Carpenter pointed out that it would be well for Mr. Hurst to publish the evidence which had led him to these conclusions, and held that if Mr. Hurst's statement as to trouble from cracked piston heads being practically absent in low phosphorous irons could be substantiated it appeared to give the required solution of the difficulties.

As a concluding instance of the satisfactory use of cast steel, Professor Carpenter instanced the large Humphrey gas pumps installed at Chingford, the combustion chambers of which are made of cast steel, and not, as in former practice, of cast iron. In view of the many practical cases put forward by Professor Carpenter one can agree with his general summary of the position as to there being a distinct movement in favor of substituting for grey cast iron some one of the many varieties of steels available in cases where engine and other parts have to withstand either alternate heating and cooling or very prolonged heating, both under atmospheric and super-atmospheric pressures. Certainly in the majority of cases this substitution appears to have been successful. —*The London Daily Telegraph.*

Diesel Engines

THERE is no room for doubt that far larger numbers of Diesel engines were constructed in 1916 than in any previous year, although it is scarcely necessary to remark that the majority were for war purposes, and especially for submarines not only for the belligerent countries but also for the navies of the neutral nations. Apart from this, however, there were some noteworthy developments in Diesel motor construction. Buyers of those engines which are to be employed in Great Britain are now demanding that they should run on tar oil, and manufacturers are making the necessary modifications. A satisfactory feature is that British stationary Diesel engines are gaining in reputation, and two or three types can now be considered as practically equal to anything constructed abroad.

Several circumstances have conspired to increase the utility and desirability of Diesel motors of low power, and the past year for the first time saw the introduction of engines from about 10 horse-power up to 150 horse-power on a large commercial scale. This development is chiefly to be noticed in Sweden, where a special factory has been erected to build Polar Diesel engines from 12½ horse-power in one cylinder to 75 horse-power in six cylinders on a standardized basis, and already scores of these sets have been built—a large proportion for Russia. —*Engineering Supplement, London Times.*

Looking out over the City of Homs, showing the character of buildings in the towns in the region traversed by the Baghdad railway

The Baghdad Railway*

An Important Trade Route by Which Germany Hoped to Control Valuable Territories

It was in the year of 1899 that the Germans commenced to build the all-important line of railroad known as the Imperial Ottoman Baghdad Railway, ostensibly the property of a company bearing the same name, but literally the enterprise of the German Government. The concession was granted to the Anatolian Railway, which was, and still is, also Teutonic property, the Ottoman Government having made the concession as liberal as possible upon the condition that it was not to be called upon to find any of the money for the construction. The track was to be 1,350 miles in length, including several branches, and was to run from Konieh, the extreme east point of the existing Anatolian Railway, to the city of Baghdad, in Asiatic Turkey (Mesopotamia), to Vasra (also called Bussorah), and thence to the Persian Gulf. The track which commences, as stated, at Konieh, runs south-east to Adana, thence east to Aintab and Berejik, where it crosses the Euphrates River upon a magnificent steel bridge, thence making its way, still east, to Mosul. Here it turns south, down the right bank of the Tigris River, and so into Baghdad. That is the program mapped out, but it will be some time yet before the ambitious plan is brought to completion.

The branches, of which there are several in construction and still others in contemplation, include one from Aintab to Aleppo and another from Mardin to Sivas.

The original concession was for the period of ninety-nine years, but a number of clauses have been inserted capable of wide interpretation and elasticity; long before the period mentioned expires the railway will probably have passed entirely out of the hands of the concessionaires and the concessionists alike. The same consideration may be suggested with regard to the length of time allowed for the complete construction of the railway. This was placed at eight years from the date of the concession, 1899, but, as will be seen, the allotted period—1907—has long since passed, and the railway is not one-half finished.

The normal gauge of 1.44 metres (=4 feet, 8¾ inches), was decided upon, since that is the gauge of the Anatolian Railway, which runs from Stambul, on the opposite side of the Golden Horn, facing Constantinople, and the idea was to run trains without any change of carriage from Constantinople to Baghdad. It was also intended by the Convention of March 5th, 1903, to place the entire railway under the control of the Germans; but to this the British Government, which had insisted upon being interested in some portion of the enterprise in view of our great economic stake in the Persian Gulf, objected. Some months later—November—a further agreement was arrived at, by which the Germans were to control 40 per cent of the capital, the French 20 per cent, the British 30 per cent, and the Turkish Government the remaining 10 per cent. The Germans at once proceeded, as is their wont, to organize a limited liability company to handle their part of the undertaking, subscribing the preliminary capital of £150,000 to build the first section, and establishing the offices of the company in Frankfort. We British, as usual, did nothing definite, but allowed the Germans to make great headway with their portion, while the French followed the British example and looked on.

The first section of the line from Konieh *via* Eregli and Bugurlu was constructed—and well constructed, too—to the length of 124 miles, the railhead being reached some time in 1904, or about five years after the work was begun. The next section taken in hand was shorter, about 105 miles, from Burgurlu to Aleppo. To carry out this portion of the work a separate small company was formed in Switzerland with German capital, the head offices being situated in Glarus. The progress made subsequently was very remarkable, work proceeding simultaneously in three different directions—the first from Adana eastward to Amanus, the second from Adana westward to the Taurus Mountains, and the third from Bugurlu eastward also to the Taurus Mountains. But the section eastward from Adana to Bugurlu has since fallen into abeyance, although the original plans called for its construction early in 1909. The greatest engineering difficulty is experienced in this part of the country, and even the surveys have not yet been finally completed. For many months—now running into years—the German engineers and surveyors have been examining the position in all directions, especially around Adana, Yenidje, Missis and Hamidieh. Up to the time of the outbreak of the war with Turkey it had not been determined whether the line, on leaving Adana, should run directly to Aleppo *via* Hamidieh-Osmanieh-Bagtche, a very costly route to follow, or *via* Alexandretta. What had been resolved upon, however, was that the line on approaching Adana should meet the Mersina-Tarsus-Adana line at a point called Yenidje, about half an hour's journey from Adana. It had also been arranged that connection was to be linked up with the Russian system of railways in Northern Persia by means of a branch to Khanakin. This part of the arrangement has, of course, been cancelled, but no doubt eventually it will be restored to the original program when—and if—the Baghdad Railway becomes jointly a British-Franco-Russian enterprise.

The originally proposed terminus, which will also now have to be modified, was to have been on the Persian Gulf at Koweit, a completely transformed town. But an alternative and far more probable route is that proposed from Homs—on the already completed French railway from Tripoli to Aleppo—and thence to Baghdad, a distance of about 510 miles, with a branch from Damascus to Palmyra—125 miles in length—Palmyra being some 85 miles east of Homs. The last section but one of the railway to be opened is that to the western side of the Taurus Mountains, a length of about 80 miles between Sammarra and Baghdad. There would then be a further distance of about 450 miles from Baghdad to Beyrout *via* Damascus to be completed in order to bring the whole of this valuable Mesopotamia district into connection with the Persian Gulf through Busra.

Notwithstanding the interruption occasioned by the war, construction on a further section of the Baghdad Railway has been proceeding, and, as recently as October 1915, the section between Islahie and Radju was completed and opened to traffic. Both of these stations are situated east of Amanus and north of Aleppo, and the completion of the section brings the Syrian part of the system nearer to the main tunnel—known as the Bagtche tunnel—by 30 miles. Here, however, there has been a halt, and it looks like being a long one. It is claimed that the great tunnel has been completely pierced, but there is reason to believe that the statement is untrue and is destined to remain unfulfilled for some time yet.

The tunnel referred to will eventually pierce the Amanus Mountains, on the border between Asia Minor

*From an article in *The Engineer*.

and Syria. With its completion the more serious part of the engineering difficulties will have been overcome, and the work of connecting up the many isolated stretches of track will follow. At the point reached shortly before that where the Bagtche tunnel is encountered, the track begins to rise from the plains towards the mountain range at a gradient of some stiffness. The rise begins after leaving Mamoure, the last station in the Cicilian Plain, the slopes being very uneven and irregular, and calling for a great number of retaining walls and masonry culverts. There are numerous deep ravines which have to be spanned by steel bridges, and of the eleven or twelve required only eight have so far been completed. There are also seven minor tunnels, with an aggregate length of 6,355 feet.

Bagtche station is reached at an altitude of 1,754 feet, and after passing this the great tunnel bearing the same name will be entered, the track then passing through two smaller piercings having a respective length of 236 feet and 394 feet, and over one short steel bridge. The Bagtche tunnel, like most of the other openings, is cut out of the solid rock, and much difficulty has been found with the water encountered in some parts. The length of the tunnel is 16,028 feet, and for about 8,000 feet of its length it ascends gradually until it reaches an altitude of 246 feet above the level of the entrance. There is then a gradual descent for a length of 3,000 feet odd, the tracks finally emerging at the other end at an elevation of 197 feet above the level of the Amanus entrance. Between the point of exit and that of the next station—Islahieh—not as yet completed, there are four other tunnels having an aggregate length of 3,500 feet. From first to last the length of tunnelling which has had to be allowed for in crossing the chain of mountains attains to 26,250 feet.

The great Bagtche tunnel has been compared for length and engineering difficulties encountered to some of the large Alpine tunnels, such as the Mont Cenis, the St. Gothard, and the Arlberg. The cost is also expected to work out at hardly less than something between the cost of the Mont Cenis tunnel, which was about £224 per yard, and that of the St. Gothard, which was £142 per yard. The Arlberg tunnel proved comparatively cheap, costing on an average but £108 per yard. Whereas the gradient in the Simplon tunnel is 1 in 500, that of the Bagtche tunnel is 1 in 394.

In tackling the Bagtche rock, which is hard quartz, special boring machines had to be constructed. In some parts of the tunnel hydraulic rock drills of the Brandt type and dynamite were used, as in the case of the Simplon tunnel. It is said that some of the expert drillers who were employed upon the Tauern tunnel, in the Austrian Alps, and the Lötschberg tunnel, completed in 1911, have found engagements on the work of the Bagtche tunnel.

It has been stated in certain German organs that this great tunnel is finished so far as the boring is concerned. This statement, however, as indicated above, seems open to serious doubt. It is certain that shortly before the war broke out there was a very considerable amount of work remaining to be done before the boring could be said to have approached its end, even in the roughest stage of the work. At that period the Karapounar-Dorak section, one of the shortest, barely more than 34 kiloms. in length, was just on the point of completion, and this comprised only one out of the four tunnels referred to. It is this section of the line which offers the most serious engineering difficulties—none, be it said, that cannot ultimately be overcome, but necessitating great patience and enormous outlays of labor and capital. At this time of the year, moreover, the whole region of the country in which work is being carried out is subject to devastating storms, heavy rainfalls, mountain torrents and avalanches, landslides and heavy falls of rock. Thus the report of "rapid progress" even now may be very seriously discounted. The whole of this part of Mesopotamia is subject to flood at certain seasons, and one of the most important stipulations contained in the specification of the Baghdad Railway is that relating to the construction of ditches, gutters and small banks along the line, such as shall be deemed necessary for the drainage of the track and borrow-pits and the maintenance of the works.

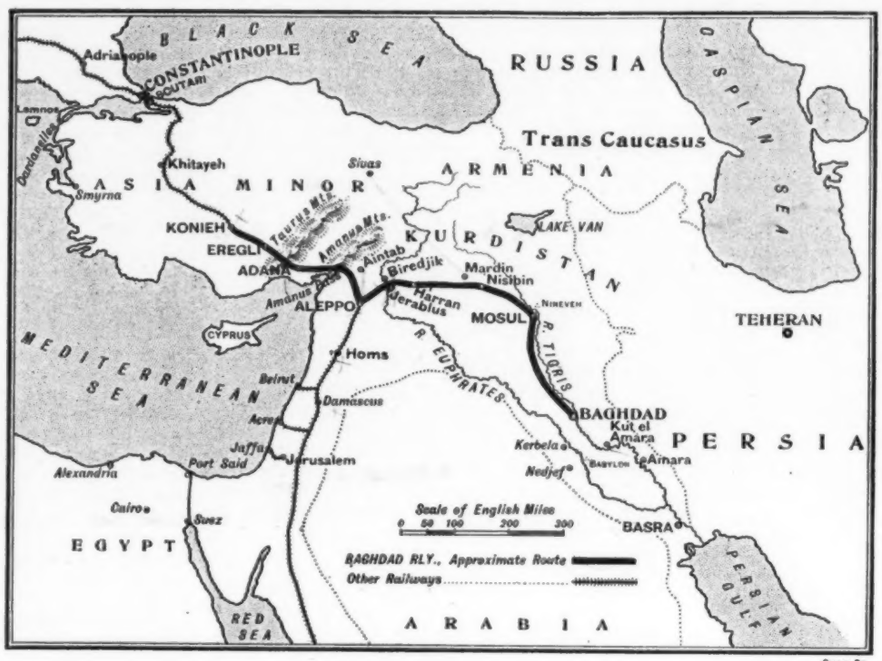
The actual line from Konieh to Baghdad, which is some 900 miles in length, is destined to serve the following towns and districts: Karaman, Eregli Kardash-Beli, Adana, Hamidieh, Osmanieh, Bagtche, Kazanali, Killis, Tel-Habesh, Harran, Ras-ul-Ain, Nisbin, Avniat, Mosul, Tekrit, Sadidjeh, Baghdad, Kerbela, Nedjef, Zobeir, and Busra. But, in addition, there are to be the following four branches: (*a*) From Tel-Habesh to Aleppo; (*b*) from a neighboring point on the main line—to be agreed upon—to Urfa; (*c*) from Sadidjeh to Khanikin; and (*d*) from Zebeir to a point on the Persian Gulf—to be agreed upon between the Imperial Ottoman Government and the concessionaires. The construction is not destined to stop even here, however, for steps have been taken, and consent has been given, to construct the following further seven branches: (*e*) Towards Marash, (*f*) towards Aintab, (*g*) towards Birijik, (*h*) towards Mardin, (*i*) towards Erbil, (*j*) from the Diala towards Salakie and Tuz-Kharmati, and (*k*) from El-Badj to Hit.

By the beginning of June of the year 1915 the following sections of the portion of the railway known as the Aleppo line had been opened and put into operation: Aleppo, Mouslemie, Akterine, Tchoban Beyi Akje Koyounli, Hulmenie Jerablus, Siftek, Ser Arab Pounar, Kharab Nass, Qul Tepe, Tell Abiad, Ghodjar, Touem, Tel-el-Hamoud, and Rasal-Ain. The journey on this section occupies ordinarily some eleven hours.

The entire length of line which it is claimed has now been laid down and opened to traffic leaves but a distance of 373 miles between Aleppo and Baghdad. But apart from Aleppo, which, it will be remembered, is a branch and not a part of the original Baghdad Railway, the distances to be covered between Konieh, Busra (or Koweit) and Baghdad are as follows: Konieh to Baghdad, (roughly) 900 miles; from Baghdad to Busra, 350 miles; and from Busra to Koweit, 100 miles. Thus we have a total distance for the track of 1,350 miles.

The other railways in Turkey include German, French and British-owned lines. These are—and in the case of the first-mentioned still remains—the Anatolian Railway, which, including the two branches to Adabazar and Angora, has a length of 650 miles; the Smyrna-Cassaba Railway, which works a line of some 320 miles miles in length, and the Smyrna-Aidin Railway, which works 340 miles. In addition there is the Mersina-Adana Railway, formerly under Franco-German management, which works about 40 miles.

It is not without interest to note that the original idea of the Baghdad Railway was British. The nucleus of the line was the short piece of track built from Haidar Pasha, opposite Constantinople, to Ismidt, which was always intended to form the head of a line to Baghdad. At first this short line was leased to a British group, in which the only other person of foreign nationality interested was an Austrian, and he held but 5 per cent in the undertaking. In the end this railway was seized and given over to a German group, the same group that afterwards obtained extensions of the railway with permission to build to Baghdad. The main idea that the railway should be an international one soon gave rise to controversy and contention, and although a feeble attempt was made to preserve British interests, from first to last German interests and domination have prevailed. Writing on the subject latterly, Sir Edwin Pears, who knows more about the internal history of Turkish politics than any other person, said: "The concession of the Baghdad Railway carried with it many smaller concessions. It is beyond doubt that to obtain them large bribes were paid to the Turkish Ministry and employees. Germany further attempted to obtain a monopoly of railway concession in north-eastern Asia Minor, but her demands justly aroused the opposition of Russia and failed. The construction of the Baghdad Railway was pushed on rapidly. The better-educated part of the population recognized its great utility, both for commercial development and for military purposes. But the work did not tend to make the Germans popular, because the mass of the population believed that the large sums assigned from the revenues as subsidies would constitute an intolerable tax."



THE BAGHDAD RAILWAY

Swain Sc.

The aggregate length of the line Haidar Pasha (Constantinople) Konieh-Baghdad, of which the section from Haidar Pasha to Konieh belongs to the (German) Anatolian Railway Company, whilst the section Konieh-Baghdad represents the Baghdad Railway proper, amounts to 1,512 miles, comprising the following sections:

	Miles
Haidar Pasha-Eski-Schehir	194
Eski-Schehir-Afian-Karahissar	100
Afian Karahissar-Konieh	103
Konieh-Kara Pounar	182
Kara Pounar-Adana	48
Adana-Islahie	95
Islahie-Aleppo	88
(Aleppo) Moslimie-Mosul	393
Mosul-Baghdad	249

According to an Austrian railway technical journal, work on the Baghdad railway was provisorily completed in February, 1916. Between Aleppo and Baghdad the double track has been finished in the sectors Aleppo-Moslemie-Ras-el-Ain and Samarra-Baghdad, and trains are now running. The railway between Ras-el-Ain Mosul and Samarra is, however, still under construction, and cannot be completed until after the war. The tunnels and cuttings between Islahie and Momure and between Dorah and Karapounar were still in an unfinished state.

A Prairie Dog Feud

A FEUD appears to have broken out among our prairie "dogs," and while there have been extensive arguments and much excitement, it seems that no real damage has been done. The animals occupying the northerly burrows of the village seemed to have declared warfare against a group living in the hillocks near the southwestern portion of the enclosure. There is much popping in and out of heads, shaking of black-tipped tails, shrill calls of squatting sentries and like maneuvers. Owing to the sharp cold of the morning hours, the fighting does not begin until well along toward noon—and then it occurs chiefly on sunny days. Then the clans issue from their respective strongholds. The method of attack consists in rushing at an adversary that has been sitting up, chattering taunts, and knocking him over backwards. A retreat by any one of the combatants is considered a decisive defeat. If a "dog" dives into a burrow, the victor joyously fills up the mouth of the hole with loose earth, and vigorously packs it down by butting the ground with the top of his head. The entombed and intimidated animal seems too ashamed to dig his way out for at least an hour or more.—*Bulletin of the New York Zoological Society.*

The Metric System*

The International Language of Weights and Measures

By George Frederick Kunz, Ph.D.

How difficult it sometimes is to make practice conform to theory has long been and still is shown by the continued opposition, active or passive, to the general introduction of the metric system into the United States and England. There can, indeed, be little doubt that so progressive a land as our own would long since have cast off the burden of the old scale of weights and measures, were it not for the fact that Great Britain, with which our commercial relations are so closely knit, still clings persistently to the so-called English weights and measures. On the other hand, could we pluck up courage enough to take the first step, England would most likely follow our lead after a shorter or longer interval.

Of the great advantages the metric system possesses in simplifying all necessary calculations regarding dimensions, there can be no question. Hence the opponents of its introduction here base their arguments mainly upon the difficulties involved in a readjustment of the various mechanical appliances of manufacture to a radically different scale. However, in many cases this difficulty is more apparent than real, and would merely imply a remarking in accord with the metric equivalent of the old measures, and the actual expense and very temporary inconvenience involved would assuredly be many times remunerated by the great advantages secured.

The first definite proposal for legislation to establish the basic principle of the metric system was made by as thoroughly practical a man as the great diplomatist Talleyrand, who in 1790 brought forward a measure in the French Assembly directing that a new unit of measurement should be established, based on the length of a pendulum beating seconds on latitude 45°; the selection of a pendulum beating seconds as a standard dates back, however, to Picard in 1671 and to Huygens in 1673. While this proposition of Talleyrand paved the way for the elaboration of the metric system, the details of the plan as worked out by the mathematicians Borda, Lagrange, Laplace, Condorcet and Monge, provided for the determination of the exact length of the quadrant of a meridian. Eventually, the ten-millionth part of this became the unit of length, and was denominated "mètre." In 1791 the Assembly sanctioned the measurement of an arc of 10 degrees on the meridian of Paris, and as a result of seven years' work by Delambre and Méchains, this task was successfully accomplished.

The weight of a cubic decimeter of distilled water, divided by 1000, gave the unit of weight, the "gramme," and the contents of a vessel of such dimensions constituted the "litre." From August 1, 1793, the metric standard has been the only one recognized in France and it has since been adopted by almost all the countries of Europe and America, excepting England and the United States, although it received legal recognition here July 27, 1866.

For the effective development of our foreign trade, for the utilization of the great and unique opportunities in this direction that the world war has given and will give us, it is most urgent that all foreign catalogues and publications issued by our manufacturers should have all dimensions expressed in metric as well as in English weights and measures. This can be done by placing the metric equivalents in parentheses. No better object lesson of the superiority of the metric system could be desired than that which would be afforded in this way, as its uniformity and simplicity would thus be brought directly home to everyone who consulted the figures.

One notable result of the great demand for war material in this country from the European nations, has been the enforced introduction of metric measurements in a large number of the factories devoted to such manufacture. This not only refers to guns, rifles, shells, etc., but also to locomotives, rails, parts of bridges, many tools and pieces of machinery, etc. The increased demand for our goods from South America, and the movement among our manufacturers to take advantage of the check of European exports to South America in order to introduce our productions there more widely and more consistently, works in the same direction. For our manufacturers are slowly learning the important lesson that if we wish to increase our trade in foreign lands we must endeavor to conform to the standards and usages current therein. When the war is over great opportunities will present themselves; but we must prepare now with a universal language of weights and measures.

England and the United States are slow in realizing the waste of time and the chances of error involved in translating the terms of a logical and consistent standard of weights and measures into those which only owe their use to a blind maintenance of tradition. For this reason those of us who favor the widest possible use of the metric system, not merely because this system is in force in all parts of Europe and America except England and our own country, but because of its inherent merits, must welcome every step taken in what we regard as the right direction for the attainment of what would prove a most potent factor in international trade, especially in Pan-America.

A not unimportant step in furtherance of the adoption of the metric system has recently been taken in the gem dealer's industry, and although this particular application may appear to many at first sight as being of comparatively slight consequence, its educational effect will be more far reaching than is generally supposed. This concerns the adoption, in precious stone commerce, of an international metric carat of 200 milligrams, to take the place of the various and discrepant national carat-weights that have for so long been a source of serious annoyance, inconvenience and loss of time for gem-dealers.

When we reflect that there are some 36,000 jewelers in the United States, and that because of the popularity of their wares they come into constant contact with a large section of our population, we can realize the good work they will necessarily perform in demonstrating to their customers the usefulness of the metric system in this particular case; and thus arousing public attention to its signal merits.

The chaotic conditions with which gem-dealers had to contend will be appreciated when we consider that there were at least eighteen different national or local carat-weights, ranging from that used at Turin, equivalent to about 3.295 grains (213.5 mg.), down to the Bologna carat of 2.91 grains (188.6 mg.). Hence the heaviest, the Turin carat, was a little more than thirteen per cent heavier than the lightest, the Bologna carat.[1] The impossibility of carrying on a diamond business systematically with such an appalling variation in the weight of the diamond carat, and with no possible means of finding an effective check to determine the accuracy of the weights employed, must be clear to all.

As early as 1893, in a paper read in Chicago before the International Congress of Weights and Measures, held in connection with the World's Columbian Exposition, I suggested dividing the carat into 100 parts, and constituting a standard international carat of 200 milligrams, that is, 5 carats, or 20 pearl grains, to a French gram. This represented a depreciation in weight of only about two and one-half per cent from the carat-weights most in use, and by the universal acceptance of the new standard all the confusing conditions that have so long obtained would be done away with once and for all. Much credit for having definitely initiated this much-needed reform is unquestionably due to Mons. C. E. Guillaume, Director of the Bureau Internationale des Poids et Mesures at Sèvres, who energetically and successfully advocated the reform in 1906, before the Commission des Instruments et Travaux, in Paris.

This new standard has now been either exclusively adopted, or officially recognized throughout Europe and America, England having fallen into line by an Order in Council, made on the recommendation of the English Board of Trade, to take effect April 1, 1914. The French law of June 22, 1909, may serve as a model of this legislation, its single paragraph containing the following concise provision:

"In the transactions relating to diamonds, pearls, and precious stones, the designation 'metric carat' may, in violation of the first article of the law of July 4, 1837, be given to the double decigram.

"The use of the word 'carat' to designate any other weight is hereby prohibited."

While this law, as well as those promulgated in many other lands make the use of the metric carat obligatory, in the United States its legalization, coupled with its official adoption and use in the Treasury Department from July 1, 1913,[2] for the customs service, and sustained by the resolutions recommending its use from the same date passed by the National Jewelers' Board of Trade, and the National Jewelers' Association, have given to the metric carat all the sanctions that an ordinance commanding its use could have provided.

Most of us are ready to admit that the use of a universal language would do much to remove the national antipathies leading to the armed conflict of nations, and the universalizing of a simple standard of any kind brings the nations nearer together and helps on a better mutual understanding. When the same signs and symbols express to all the same weights and measurements, this will mean a distinct advance along the road leading to international peace and good-feeling. Whereas all the world can learn readily the words mètre, gramme and litre, how many of us know the words for inch, foot, yard, rod, furlong, mile, league in more than two or three languages; yet the term mètre would cover all these and we could all understand what is meant, and accurately.

Our adhesion to the metric standard should be encouraged when we consider that as early as May 20th, 1790, Thomas Jefferson, as Secretary of State, formulated a decimal system of weights and measures, and embodied the scheme in a report. The adoption of the decimal system in our coinage, already urged by Gouverneur Morris in 1782, probably caused Jefferson to favor its extension to weights and measures as well.[3] Therefore, in adopting the metric system we would only be realizing one of the brilliant and inspirational ideas of the most original thinker among the founders of our republic.

The foregoing testimony plainly shows that the time is ripe for this country of ours to give serious consideration to the question of joining with the other progressive nations of the world in doing business on a metric basis. Is not this the place to take preliminary action toward bringing about this needed reform? We have here representatives from many lines of commercial endeavor, each of whom believe that metric legislation must eventually obtain in this country. It would be a lasting pity if before we adjourn this evening, we do not form some sort of an organization whereby the several metric committees represented at this gathering, as well as individuals interested in the metric cause will be held in touch with each other. Such an organization should be dedicated to the promotion of metric education and legislation along rational lines.

Our first step would be to bring the commercial organizations of this country to a realization of the fact that our present system of standards is injurious to our foreign trade and is wasteful of vast amounts of our most precious commodity—time. This accomplished, we should proceed to urge legislation along such lines that will bring the needed reform to those who wish it with the least amount of annoyance to those who for their own reasons oppose it.

I believe it entirely feasible to arrange legislation, either by optional use of the two systems of weights and measures for a certain term of years, or by temporary exemption of certain types of machinery manufacture, so that the change can be made with a minimum amount of loss or trouble. I feel that such work is a patriotic duty of all of us present this morning. I believe that we have right now an unusual opportunity for making the metric system, the American, as well as the International Language of commerce.)

Inferences Concerning Auroras

THE following correction relating to an article upon the above subject, which was reproduced in the issue of the SCIENTIFIC AMERICAN SUPPLEMENT of March 24, No. 2151, appears in the March issue of the Proceedings of the National Academy of Sciences:—

Professor Alexander McAdie has kindly drawn my attention to an error in the dates occurring in my paper, Inferences Concerning Auroras, published in the Proceedings for January, 1917 pages 1–7. In reference to the "Aurora of April, 1883", he has convinced me from his data that the particular event occurred on November 17, 1882. My mistake was due to faulty references, and in no way affects the general argument.

ELIHU THOMSON.

*A paper read before Section I, Social and Economic Science of the American Association for the Advancement of Science by its retiring chairman, December 27th, 1916.

[1] "The Book of the Pearl," by George F. Kunz and Charles H. Stevenson, p. 323, New York, 1908.

[2] "The New International Metric Carat of 200 Milligrams," George Frederick Kunz, Trans. Amer. Inst. Mining Engineers, New York Meeting Feb. 1913 (also the Meeting Aug. 1913), pp 1225-1245.

[3] W. Stratton, Director, United States Bureau of Standards. Encyclopaedia Americana, art. Metric System.

The Fixation of Nitrogen as Cyanide*

An interesting lecture was delivered by Prof. John E. Bucher of Brown University, Providence, R. I., on this subject on the afternoon of March 2d, at the College of the City of New York. Professor Bucher described in the most vivid manner his experiments on the production of sodium cyanide by passing a current of nitrogen over a mixture of soda ash, coke and iron filings. The lecture covered much the same ground as his recent paper before the American Institute of Chemical Engineers, abstracted in our issue of January 15th, page 82. But a fuller account of the work of Professor Bucher in this field should prove particularly interesting to our readers at the present time.

The address was given in the chemical lecture room of the college and Professor Bucher had considerable apparatus set up to show how the various processes described were conducted. The time did not allow of the performing of but one or two experiments, but the apparatus, together with many samples, served to elucidate the description of the processes. After the lecture Professor Bucher answered many questions and explained some of the apparatus in greater detail.

The reactions which were studied by Professor Bucher in his work are not new, and he first took up the history of the subject. Lewis Thompson, in 1839, described in the "Mechanics Magazine" (London) experiments made on the manufacture of cyanide and its conversion into Prussian blue. He ground two parts of pearl-ash, two parts of coke and one part of iron turnings into a coarse powder and heated this in an open crucible to a full red heat for half an hour. He obtained potassium cyanide from this, which he converted into Prussian blue. Particular attention was called by Professor Bucher to the fact that Thompson stated that iron was necessary in the process if it was to be carried out at a moderate temperature. In 1843 Newton took out a patent for the production of cyanide, using atmospheric nitrogen. A factory was built at Newcastle-on-Tyne which produced yellow prussiate of potash for some time, but finally failed in 1847 owing to the non-use of iron and the consequent necessity of operating at a very high temperature.

Victor Alder of Vienna took out patents in Germany in the years 1881 to 1885, in which it was stated that iron could be used but that it was not absolutely necessary to use it, and that the process would work all right without iron. Attempts to work the process according to these patents for several years in Germany finally resulted in failure. Thus the original condition for successful working as originally established by Thompson was overlooked in the practical work following.

Professor Bucher mentioned that he had done work on a magnesium nitride process, forming the nitride from metallic magnesium and nitrogen. This when heated with ammonium chloride gives ammonia and magnesium chloride. He did not, however, go into this in any detail.

The first experiments discussed were those on the fixation of atmospheric nitrogen with alkali metal according to the equation:

$$2Na + 2C + N_2 = 2NaCN + 46{,}200 \text{ calories.}$$

A ½-inch malleable iron pipe about thirty inches long was inclosed in a larger horizontal pipe so that the inner pipe could be surrounded with hydrogen or nitrogen to prevent diffusion of carbon dioxide into the inner tube. This inner tube was filled with lampblack and heated to a white heat in a current of hydrogen. Sodium was then placed in one end of the tube and nitrogen passed through and allowed to bubble through water in a flask at the other end. If nitrogen is absorbed, then a partial vacuum is created and the water will rise in the tubing toward the iron tube. In the experiments with metallic sodium and lampblack or graphite the water rose very slowly, indicating a slow absorption of the nitrogen. It was found to require hours to get much absorption.

Using the same apparatus and adding finely divided iron to the charge made a marvelous change in the rate of nitrogen absorption. In this case the water backed up so rapidly that there was scarcely time to turn the nitrogen on full force to keep the water from entering the tube and reacting with the sodium. In this case 94 per cent of the sodium was converted into cyanide and fused globules of iron were found in the tube, indicating the strongly exothermic nature of the reaction. The catalytic action of iron was thus seen to be extremely strong.

This experiment led to the testing of this process for the purification or decarbonization of iron. Professor Bucher showed several small hack-saw blades which had been heated in a current of nitrogen and sodium vapor to a dull red heat for several hours. They were very soft and silvery in appearance and could be bent into any shape without breaking. He then reheated one of the blades to a good red heat and quenched it. It still remained soft, thus showing the absence of carbon. He then made one of these blades the anode in a beaker of dilute hydrochloric acid. The blade remained silvery white and no carbon was deposited. On making an untreated blade an anode in the same solution and turning on the current a few minutes, carbon was quickly deposited on the bottom of the beaker. The sodium was stated to remove not only carbon but also phosphorus and sulphur.

The utilization of this reaction for the preparation of carbon-free metallic iron was also discussed in connection with the reversibility of the equation.

$$2NaCN + \text{Iron} \rightleftarrows 2Na + N_2 + \text{Carburized iron.}$$

This is the equation for the case hardening of iron, and when experiments were made with a mixture of sodium cyanide and pure iron powder and heated in an iron tube considerable metallic sodium was found in the colder part of the tube.

The next set of experiments discussed were those in which sodium carbonate was used instead of metallic sodium. In experiments made without the use of iron with a mixture of powdered graphite and sodium carbonate, heated in a ½-inch copper tube at 920 degrees C in a current of nitrogen, only a very small amount of sodium was formed and no cyanide. The fact that sodium was found, however, indicated that sodium carbonate and carbon do react to give metallic sodium.

Following these experiments, tests were made using mixtures of graphite, iron and sodium carbonate and heating in the ½-inch tube in a current of nitrogen at 920-940 degrees C., as before. In this case the product gave 60 per cent of the sodium carbonate converted into cyanide. These experiments with iron showed that Thompson's original statement that iron was necessary is correct and that the cause of failure of subsequent work was the neglect of this condition. From the results of a large number of tests on pipes of many lengths and diameters, it was found that 2-inch pipes 10 feet long were the most efficient.

The inconvenience of using powder led to the development of a system of briquetting, in which advantage is taken of the solubility relations of sodium carbonate. Hot water is added to the mixed charge of coke, soda ash, and iron. The maximum temperature of a sodium carbonate solution is 104.75 degrees C. The percentage of carbonate (about thirty per cent), in the solution does not change much until the temperature is lowered to about thirty-five degrees. Then $Na_2CO_3 \cdot 10H_2O$ crystallizes out rapidly down to about twelve per cent Na_2CO_3 at 10 degrees. The mass is mixed with the water at as near the maximum temperature as convenient, thus giving a range of 70 degrees C. through which the mass might cool before the water would be taken up by the crystals to form the dry hydrate, $NaCO_3\,10H_2O$. Many tons of briquets were made in this manner. At first an ordinary concrete mixer without steam jacket was used, but later a steam-jacketed kneading machine was found more convenient. Small briquets were made from the hot mixture by a small meat chopper, such as found in every household. One of these will produce 5,000 pounds of briquets in 24 hours. The briquets should be dried rapidly so as to get them hard. Hot waste gases are good for this purpose. On a commercial scale, iron scale was used and ground to 100 mesh in a ball mill. One hundred mesh coke was then added and then soda ash. With these briquets charges were put through which ran 28 per cent NaCN when finished.

Experiments were then made to determine the percentage of soda ash which would give maximum commercial efficiency and not cover the catalytic iron surface so as to render it sluggish. Three tests were made with the iron-coke-soda ratios 2:2:1, 1:1:1, and 1:1:2, respectively. These experiments showed that the 20 per cent soda ash briquets were the most reactive and that the reaction diminished as the soda ash was increased beyond that point. Also a higher temperature is required for the higher soda ash content.

In conducting experiments with the tube horizontal it was found that the briquets often sagged, forming a channel over the charge. The nitrogen upon being introduced quickly found this channel, and consequently there was little conversion to cyanide. This difficulty was overcome by placing the retorts or tubes vertically.

In connection with the question of loss of alkali by volatilization, experiments were made with continuous oil-burning furnaces with continuous feed of the charge in at the top and continuous withdrawal by conveyor at the bottom. Trouble was experienced by the plastic briquets hardening in the conveyor so that it could not be turned. The reaction tube was made longer so that the worm conveyor could be placed about four feet below the bottom of the furnace. This gave the briquets a chance to cool and they passed through the conveyor all right. They stuck to the pipe, however, in descending, and consequently it was found advisable to use an enlarged section of pipe for the lower end of the furnace, or to cool this lower end.

In taking up the discussion of electric heating of the charge, Professor Bucher showed several tubes in which electric heating had been done. The first one with which he experimented was a section of 1½-inch galvanized iron pipe about three feet long and with copper bands on each end. In electrical heating a resistor outside the tube may be used, the tube itself may be used as resistor, or the charge may be used as resistor. Professor Bucher says the ideal way to heat the charge if it is done electrically would be to use the charge itself as resistor. With the above-mentioned galvanized iron pipe as resistor and furnace combined, a test was made with the pipe surrounded with magnesia asbestos to prevent oxidation. The zinc rapidly burned off the furnace, when the current was turned on, but the test worked satisfactorily and the resulting briquets gave 22 per cent sodium cyanide.

With an apparatus rigged up with a 15-kw. welding machine in place of a transformer and using a 4-inch iron pipe 6 feet long as the resistor and furnace combined, charges were put through which gave 25 per cent NaCN consistently. Professor Bucher had one of these welding machines on exhibit. Vertical iron wires surrounding the furnace and suitably spaced from each other have also proven successful as resistors when properly protected from oxidation. Professor Bucher had a set of these wires connected up and showed how they heated up with the current on. The resistance increase could be plainly seen from a large ammeter on the wall as the current dropped on rise of temperature.

An interesting experiment was described with molten iron as follows: A cylindrical electric furnace with a bottom like a Bessemer converter contained molten iron and coke fragments pressed down from the top by the top electrode. Sodium vapor and nitrogen were blown in at the bottom and cyanide distils out at the top. The only drawback is the necessity of ashless carbon.

Air may be used instead of nitrogen by first passing it over hot coke and fixing the oxygen as carbon monoxide. The coke may be placed as a layer in the furnace with the briquets. Similarly, producer gas could also be used. Experiments with air were entirely successful.

Distillation in a copper tube at 1,000 degrees C. and 2 mm. pressure gave good results. The great promise of this method is threatened by the ash from the coke accumulating with each repetition until it stops the process. Ashless carbon is, therefore, necessary. It was thought that the reaction $2CO \rightleftarrows C + CO_2 + 38{,}080$ calories might be taken advantage of. At 1050 degrees C. this reaction gives almost pure CO, while at 500 degrees C. it is almost reversed.

Experiments along this line brought out the fact that with iron as a catalyser the reaction proceeded with the formation of C and CO_2 with great rapidity. It gives ashless carbon in the form of lampblack and also 38,080 calories of heat.

With electrical heating the oxidation of the iron pipe is prevented by keeping oxygen away from the outside of the pipe. With fuel-heating the problem of oxidation must be seriously considered. Thousands of experiments have shown, however, that no difficulty need be experienced from this source if proper recognition is given to the proportions of CO and CO_2 present. If producer gas is used, hydrogen might be admitted with the gas. Copper tubes would also avoid oxidation and nickel sheathing has also held out some promise.

Among the various compounds which can be made from the cyanide is sodium ferrocyanide, which can be made by adding hot water to the cyanized briquets to make a stiff paste and then steaming them in an agitator for several hours. The mass is then filtered and the black powder turned into the process again. The filtrate deposits ferrocyanide crystals out on cooling.

By adding caustic soda to the sodium cyanide solution and boiling, ammonia and sodium formate are successfully made. Other products mentioned by Professor Bucher as offering possibilities are urea and oxamid.

*From a Report in *Metallurgical and Chemical Engineering.*

An Industrious Elephant

The Indian elephant, Luna, has developed a collecting tendency which greatly embarrasses her keepers. She discovered several iron girders, each weighing about five hundred pounds, that our iron workers had stored between two fences surrounding the elephant yards. By strenuous trunk work she managed to roll these under the corral fence and drag them, one at a time, into the building. It is a heroic task to bring even one of these girders in the Elephant House, and Luna has announced each event by raising the girder and dropping it with a tremendous crash upon the cement floor. We now will have the really formidable task of carrying out the bars, as Luna cannot be induced to reverse her order of proceeding.—*Bulletin of the New York Zoological Society.*

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Fig. 1

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Fig. 2

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Fig. 3

Figures 1 to 5 are views of Lassen Peak showing progressive stages of the eruption of October 6, 1916, taken at ten-minute intervals from a point five miles distant

An Eruption of Lassen Peak*

Meteorological and Seismological Considerations

By Andrew H. Palmer, Observer, A.M. (Harvard)

LASSEN PEAK, the only active volcano within the United States, is located in the northeastern part of California, latitude 40° 30′ N., longitude 121° 30′ W., at a distance of 210 miles north-northeast of San Francisco. Rising 10,437 feet above sealevel, it is a conspicuous feature of the landscape. It is the southernmost peak of the Cascade Range, and like most of the mountains forming that range, it is of volcanic origin. While some geographers consider it a part of the Sierra Nevada, its origin and structure, as well as its position, justify its inclusion with the Cascades. These peaks have all been active volcanoes recently, geologically speaking, though measured in terms of years they have long been dormant.[1] Judging from the erosion on the sides of its old cone, Prof. R. S. Holway, of the University of California, believes that Lassen Peak had been quiet for a thousand years preceding its present period of activity. However, Cinder Cone, 10 miles to the northeast, and the Chaos Crags, at the northwest base of Lassen, have been in eruption as recently as 200 years ago.

On May 30th, 1914, residents in the vicinity of the Peak were astonished by the appearance of smoke and steam rising from its summit. An investigation made the following day by a ranger in the United States Forest Service revealed the fact that a new crater, 25 by 40 feet, had been formed within the old crater, and that the products of the eruption, consisting of dust and bits of rock, were scattered upon the snow for a distance of 300 feet from the new vent. More eruptions followed, their violence increasing and the size of the crater growing with each successive outburst. The activity culminated with the two great eruptions of May 19th and 22d, 1915. While eruptions have continued sporadically ever since, the climax of the present period of activity seems to have been passed. Outbursts continue to become fewer and less violent, much to the disappointment of scientific observers, but greatly to the satisfaction of the residents of the region. A total of about 225 eruptions have been observed. The eruptions during 1916, to July 15th, appear to verify the prediction made at the beginning of the year by J. S. Diller, of the United States Geological Survey, that as an active volcano Lassen Peak is again on the decline. Figures 1-5, inclusive, are photographs taken at 10-minute intervals by Mr. Chester Mullen, at a distance of 5 miles from the summit. They show various stages in a typical eruption, that of October 6th, 1915.

Certain phases of the present period of Lassen Peak's activity have been investigated and described by authorities in their respective branches. The geological and the physiographical aspects have naturally received the most attention. The physics and the chemistry of these eruptions have been investigated by representatives of the Carnegie Institution of Washington, but their report has not yet been made public. The meteorological and the seismological aspects have apparently been neglected, though many incidental references have been made to them. Certain facts have been observed which are at least of interest, if not of importance, in these two fields.

Photo by E. E. Stinson

Fig. 6. This view, taken from Red Bluff, Calif., May 22d, 1915, marked the climax of the present cycle of activity of Mt. Lassen

As already indicated, opportunities of this kind are infrequent in the United States, compared with the average life of man, and it is quite probable that another such opportunity will not occur during the present generation. Feeling that certain considerations are worthy of record, the following observations have been collected from all available sources.

METEOROLOGICAL CONSIDERATIONS

The meteorological aspects of a volcanic eruption are necessarily external to the crater, and are involved principally with the matter emitted. In the case of Lassen Peak certain common forms of matter are known to have been present. The dust and ash consisted entirely of rock fragments, pulverized as by great pressure, and showed no evidence of combustion such as might produce residual cinders. These fragments varied in size from microscopic bits to a mass 15 feet in diameter and weighing more than 60 tons. It appears that steam at high temperature accompanied most of the observed eruptions, and this on condensing formed the visible water vapor which when soiled by the dust particles gave the appearance of smoke. On mixing with the surrounding air this mass cooled, part of the water being precipitated as rain. Following the first eruption, that on May 30th, 1914, icicles formed on the projecting rocks on the inner side of the crater. While Diller believes that a considerable volume of water in the form of steam was ejected, the phenomenon was in no sense a geyser, and there is no evidence of surface erosion due to excessive precipitation. Such rainfall must have been very local, perhaps limited to the slopes of the peak itself. Flames[1] were reported to have been seen at various times, but it is believed that on some of these occasions the reddish color was due to sunshine reflected by clouds. In addition to the dust and water vapor ejected it is probable that there was considerable sulphur in the gaseous state. The latter is characteristic of a volcanic region. Even the fumaroles, which are so common in the great lava fields which form the northeastern part of California and a portion of Oregon and Washington, give off gases containing sulphur fumes. It was therefore to be expected that Lassen Peak should eject sulphur gas. The quantity must have been enormous, for on one occasion, when a northerly wind was blowing at the time of an eruption, sulphur fumes were detected 15 miles to the south, and there are no sulphur fumaroles in that direction. Sulphur has been and still is being deposited in a solid state on the inner portions of the crater which are in contact with the charged vapors. That the escaping gases were not poisonous, was shown by the survival of a party of 10 men who were surprised by an eruption while peering into the crater on June 14, 1914.

The temperature of the escaping material is interesting meteorologically because of the effects produced. Observers testify that some of the matter ejected in the great eruption on the night of May 19th, 1915, was luminous,[1] a condition requiring a temperature estimated at 600° to 900° C. Part of the superheated gas and ash escaped

*From the *Monthly Weather Review*.

[1]For historical data relating to volcanic eruptions in the United States consult: *Whitney, J. D.* The United States. Boston, 1889. pp. 113-116.

[1]As the nearest observers were 20 miles away flames could not be distinguished with any degree of certainty. Much more definite reports, well authenticated, state that flashes of light and cloud glows were visible and that ejected luminous bodies were seen flying through the air.—*J. S. Diller.*

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Fig. 4

from beneath the lava cap and did serious damage on the northeast side of the mountain.[3] The sudden melting of the snow produced a flood which destroyed everything in its path of 10 miles in length and exceeding a mile in width. Forest trees were uprooted or broken off at the ground and distributed in parallel rows at the bottom of the slope. Other trees, not destroyed by the rush of water, were seared by the hot blast. The United States Forest Service reported that two forest fires were thus kindled, probably the only cases of such origin which have occurred in the United States since the Forest Service was originated.

The principal fact of meteorological interest associated with an eruption is that related to the wind velocity and direction aloft. Particles freely suspended in the air necessarily travel with the wind, that is, at the same velocity and in the same direction as the surrounding air. An eruption, therefore, produces a graphic cross section of the atmosphere, as far as wind velocity and direction are concerned.

On this occasion, Mr. N. M. Cunningham, Observer, United States Weather Bureau, at Red Bluff, estimated by means of a nephoscope that the top of the visible column reached at least 20,000 feet above the summit of the mountain, or approximately 6 miles above sea level. The photograph was taken from Anderson, 50 miles distant from the volcano, and the camera was pointed eastward. The top of the smoke column was observed as far distant as Marysville, 80 miles to the south. The mushroom-like structure of the column shows the relatively stagnant condition of the atmosphere, with little or no horizontal movement. At Red Bluff at 5 P. M. on that day the wind was south, with a velocity less than 6 miles per hour. Though it can not be noted from the photograph, the wind aloft had a westerly component. Some of the dust which had risen to the higher levels was carried eastward as far as Winnemucca, Nev., a distance of about 200 miles. On that day the weather of northern California was dominated by a HIGH whose center was off the coast. The cross section is therefore one of anticyclone. On June 26th, 1914, when a slight eruption occurred under conditions of absolute calm, the dust was apparently precipitated within a radius of a mile. It is apparent, therefore, that wind is the principal element which determines the distribution of dust and ash resulting from a volcanic eruption. Moreover, the wind on certain occasions stirred up the dust, ash, and snow on the sides of the peak, and gave a false impression that new craters had been formed. Fig. 6 is a photograph taken by Mr. R. E. Stinson in Red Bluff. It is another view of the great eruption of May 22d, 1915, and was taken shortly after the westerly wind aloft had begun to destroy the gently rounded edges of the column. A second puff of smoke is seen rising in the column, most of that produced in the first outburst having drifted eastward.

Though it is apparent from the photographs that considerable quantities of material were ejected into the atmosphere by the numerous outbursts of Lassen Peak, there has been no widespread production of a volcanic dust veil, or high haze, such as followed the great eruptions of Krakatoa, Mont Pelée, and Katmai. Compared with the latter, the activity of Lassen Peak has been relatively slight. The effects upon the atmosphere have been local, and even the dust veils have been limited to 200 miles. No effect upon the weather was apparent, though the uninformed have attempted to explain various abnormalities in California weather during the past two years as being due to the activity of Lassen Peak. Moreover, there was no evidence of suction or cyclonic effects produced by the drawing in of air laterally to take the place of highly heated air rising vertically, such as is occasionally observed over forest fires. In Fig. 6 it is apparent that even the cumulus clouds, which form every calm summer afternoon in the mountain regions, were undisturbed by leteral currents.

[3] The hot ashes falling on the snow on all sides of the peak melted the snow, as at the head of Lost Creek, producing small flows of mud in all directions down the upper slopes; but *disaster occurred only where* the blast of hot gas was shot from beneath the new lava lid down the northeast slope into the great mass of snow that had accumulated for years, and instantly converted it into a deluge.—*J. S. Diller.*

SEISMOLOGICAL CONSIDERATIONS

In the popular mind earth quakes and volcanoes are closely associated. However, there is still some disagreement among authorities as to the nature of the relation. While in the Hawaiian Islands volcanoes and earthquakes are intimately associated, the Milne-Omori investigation of 8,300 Japanese earthquakes occurring between 1885 and 1892 showed little relation between earthquakes and volcanoes. Only about 3 per cent of Japanese earthquakes are of volcanic origin. Traditions among the North American Indians associated earth tremors with eruptions of the now dormant Cascade volcanoes. On the other hand, Humboldt found a tradition among the natives of South America to the effect that so long as the volcanoes in their neighborhood were active, no danger from earthquakes need be feared, but if they remained quiescent for a long-continued period severe earth shocks might be anticipated. In a manner the volcano acted as a safety valve, according to the tradition. The presence of an active volcano in California naturally invited an investigation to determine whether or not it was related to earthquakes.

Of the 225 or more observed eruptions of Lassen Peak to date, there is no authentic record of a single sensible earthquake occurring in northern California simultaneously with such an outburst. Press dispatches at various times have announced the occurrence of destructive shocks at the times of these eruptions, but when subsequently investigated none of these reports could be verified. Shortly before the first outburst of Lassen Peak, in May, 1914, a moderate shock occurred in the vicinity of Lassen; two other shocks occurred during 1915. This is about the average frequency, and since none of these occurred on days of eruptions it is believed that they were of tectonic origin, like most California earthquakes. United States forest rangers on duty in the vicinity of Lassen at the times of eruptions heard the escaping steam and the falling stones, but reported no rumbling or subterranean noises, nor did they feel any earthquakes. A letter recently addressed to the United States forest supervisor at Mineral, 10 miles from Lassen Peak, brought the following reply:

> Several very slight earthquake shocks were felt here last summer (1915) and in 1914. They did not, however, occur simultaneously with eruptions of Lassen Peak, and to the best of our knowledge there is no relation between earthquakes and volcanic eruptions in this vicinity. It is true, of course, that the ground in the near vicinity of the mountain is felt to tremble slightly during a violent eruption, but it is doubtful of this is a true earthquake shock.

It is probable that the latter tremors are simply local vibrations similar to those produced by the passage of a railroad train and are therefore not earthquakes in the usual sense.

The cause of the present activity of Lassen Peak is now believed to be the immeasurable pressure of expanding lava rising from below and not the explosive action of steam, as in the case of some other volcanoes. Prof. Holway finds it difficult to conceive of steam pressure uplifting so evenly a broken and jagged mass of rock. Diller believes that, although the visible activity is limited to near the summit of the peak, the new lava was forced in a hot, viscous state to the surface, where it spread and overflowed the crater rim. Hot springs and solfataras in the vicinity of the Peak show no evidence of increased activity.

CONCLUSION

The present cycle of Lassen Peak's activity as a volcano appears to be about complete. While future eruptions will be observed with interest, it is believed that they will be relatively feeble and infrequent. An active volcano in the United States is such a rare phenomenon that it is worthy of careful observation from every point of view. While an active volcano is of primary interest to the geologoist and the physiographer, it is of at least secondary importance to the meteorologist and the seismologist. Progress in every field demands the recording of all observations available. Those facts of peculiar interest in meteorology and seismology have been collected and reassembled in the foregoing.

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Fig. 5

BIBLIOGRAPHY

Recent articles on the present activity of Lassen Peak have been published by the following:

(1) *W. H. Storms* in Mining and Scientific Press, July 25, 1914.
(2) *R. S. Holway* in University of California Publications in Geography, v. 1, No. 7, August, 1914, pp. 307-330, plates 32-36.
(3) *J. S. Diller* in Bulletin of the Seismological Society of America, v. 4, No. 3, September, 1914.
(4) *R. S. Howlay* in Popular Science Monthly, March, 1915.
(5) *J. W. Rushing* in The Dam, July & September, 1915.
(6) *R. S. Holway* in Sunset Magazine, August, 1915.
(7) *J. S. Diller* in Bulletin of the Seismological Society of America, v. 6, No. 1, March, 1916.
(8) *J. S. Diller* in Science, v. 43, No. 1117, May 26, 1916.
(9) *Arthur L. Day*, Geologic history of Lassen Peak.
J. S. Diller, Volanic phenomena at Lassen Peak.
Abstracts in Journal, Washington Academy of Sciences, June 19, 1916, 6:404-406.

Its physics and chemistry are now being studied by A. L. Day and E. S. Shepherd, of the Geophysical Laboratory, Carnegie Institution of Washington, and will be described in a forthcoming report.

The striking photograph on the front page of this issue shows one of the picturesque forms assumed by the vapors.

An Explosion Effect

A RECENT explosion has, like all other similar occurrences been productive of many curious results, but one that I have noted seems worth special mention. There is a row of large houses in an exposed situation, directly facing the center of explosion, but about three miles from it, and in front of one of these houses is a medium-sized pond. In this row most of the houses have escaped, only two or three broken or cracked windows being noticeable among the lot, with one exception. That exception is the house facing the pond, which, so far as glass is concerned, is wrecked. From appearances it might have been played over with a machine-gun. One house, near and also facing the pond, has only one window damaged, but in this case the sashes are detroyed as well as the glass. It appears that nearly every window situated on a line crossing the pond from the explosion center has suffered extra violence. I believe similar results have been noted before over water, but this seems a very striking instance. I should perhaps add that the glass destroyed was of indifferent crown quality, whereas the other houses appear all to have thin plate-glass; but much crown glass has escaped damage in other positions, where heavy plate has gone to pieces, so I think it clear that the pond, and not the quality of the glass, was the contributory cause.—C. WELBORNE PIPER *in Nature.*

Progressive Oxidation of Cold-Storage Butter*

An Investigation of Causes of the Development of Undesirable Flavors

By D. C. Dyer, Chemist Dairy Division, Bureau of Animal Industry

MUCH has been written concerning the changes occurring in butter. The word "change" is here used in its broad and general sense to include any perceptible alteration whatsoever, although it refers principally to an organoleptic one, whether induced by one or several factors.

Butter has been kept for certain periods of time during which it has been exposed to the action of various decomposing and disintegrating agencies, and a study of the products of change thereby resulting has led investigators to draw conclusions relative to the causation of the "off flavors" so often found in stored butter. As a general rule, the majority of opinions advanced in accounting for the deterioration of butter seem to have been based either upon insufficient analytical[1] data or upon a study of butter or butter fat kept under conditions which prevail only to a very limited degree when butter is stored.

Many investigators confined their attention to a study of the fat of butter alone and sought to attribute the appearance of undesirable flavors in whole butter to some change which this one constituent undergoes. However, more recent investigations carried on with fats other than butter would appear to render such an assumption doubtful and would seem to make imperative more conclusive information concerning the causation of disagreeable flavors in whole butter held in cold storage.

The early literature in regard to the chemical changes which take place in butter is voluminous, but it is also conflicting and confusing, a great deal of it being of a purely speculative nature.

A great variety of bodies, products of chemical change, have presumably been identified in butter kept under varying conditions. The confirmation of the presence after a certain interval of time of such substances in fats known to have been originally pure is of value; yet such data obtained in the investigation of a material containing other constituents as well are obviously not so satisfactory unless it is definitely known that these attendant components do not likewise undergo similar changes. Acids, aldehydes, alcohols, and esters, among other things, may have been identified in spoiled fats, and even up to the present time it has been customary to attribute their origin solely to the fat itself. The reason for such deduction is evident. It is well known that the fat of butter is in itself a most complex material. It is a composite, made up of mixtures of the glycerids of fatty acids. Among the saturated glycerids butyrin is an essential ingredient, although palmitin and myristin predominate. Olein has generally been considered to be the only unsaturated glycerid in butter fat, yet quite recently Laxa and Koneeny[1] claim to have found that the fatty acids of the "liquid fat" of separator slime consist of 49.65 per cent of erucic acid and 21.24 per cent of oleic acids; but this assumption may not be entirely justified.

The improbability of any chemical change occurring in the saturated glycerids of storage butter is quite generally recognized; consequently the glycerid olein, purely because it contains an unsaturated linkage in the molecule and absorbs the halogens with avidity, has been considered as the source from which are derived those decomposition products the presence of which in fats influences their more or less decreased value. As a matter of fact, any satisfactory and conclusive evidence that the olein of butter fat is readily susceptible to oxidation under conditions similar to those prevailing when butter is stored is entirely lacking. On the other hand, it has been demonstrated that pure olive oil, the liquid glycerids of which consist almost entirely of olein, shows very little absorption of oxygen as measured by the iodin number, even after having been kept for three years under ordinary conditions[2]. Masters and Smith,[3] in preliminary experiments with butter fat, found but little change in the iodin value during cooking experiments carried out with this material. To obtain any pronounced change in the iodin value and in the acidity, they found it necessary to heat their samples of butter fat to as high a temperature as 200° C. while passing oxygen through the material, the mere heating of the fat to such temperature under ordinary conditions proving to be insufficient. From these two illustrations, as well as from more recent work done by other investigators, the discussion of which owing to limited space is omitted, it must be concluded that the possibility of the olein of butter fat undergoing an appreciable oxidation caused by the small quantity of atmospheric air inclosed in a package of butter is very remote, especially when it is remembered that butter is stored in the dark at a temperature considerably lower than the freezing point of water.

The inability of chemists to judge the quality of an edible fat because of the absence of satisfactory chemical data has been frequently pointed out, and this is attributable primarily, to the lack of appropriate and comprehensive analytical procedure. For instance, rancidity has generally been regarded as the natural concomitant of acidity, yet a pronounced rancidity may have appeared in a stored fat without the manifestation of any increased acidity as measured by a simple titration. Again, an undue significance may be attached to a slight decrease in the original iodin number of the fat. Such a decrease is usually considered to be caused by the taking up of oxygen by the double bond of the unsaturated glycerid; yet it must be remembered that self-polymerization—the interlocking of two or more molecules of the unsaturated glycerids—may occur, a condition which would likewise bring about a lowering of the iodin value. Again, the olein of butter fat may not exist entirely as the normal glycerid, and it is possible that a certain amount of this glycerid may occur as an isomerid. So far as is known to the writer no work has been carried out to determine whether the olein of stored butter is present entirely as the normal glycerid. In this connection it may be observed that the work of Ponzio and Gastaldi[4] and of Fokina[5] indicates that the farther the double bond is removed from the carboxyl group the nearer the iodin number approaches the theoretical value. Normal oleic acid gave the theoretical value of 90. On the contrary, 2-3 oleic acid gave a Hübl number of only 6.6, Wijs 20.4, Hanus 1.9. While there are no data at hand at present to prove that 2-3 oleic acid actually does occur in butter fat, yet this contingency is quite possible; and it is well to take it into consideration as yet another factor which may produce a slight lowering of the iodin number of stored butter. It is evident, however, that the customary methods in vogue to determine the quality of fat leave much to be desired.

One of the factors so often construed as influencing the appearance of undesirable flavors in a fat is the nature of the impurity, or impurities, contained therein. In just what manner these foreign substances bring about these undesirable characteristics has not been fully cleared up, because it is conceivable that it depends upon several parallel progressing chemical reactions and because it is possible that slight chemical changes really difficult of identification by analytical methods suffice to produce the above-mentioned disagreeable features.

It is apparent that even at the present time there seems to be considerable doubt as to whether the undesirable flavors of storage butter arise from a decomposition occurring in the fat itself or in some one or more of the other components entering into the composition of the whole product. For this reason it is thought advisable to confine the preliminary work on this subject to an attempt to settle this most basic consideration before proceeding with the further investigation of the causation of the "off flavors" so frequently met with in storage butter.

STATEMENT OF THE PROBLEM AND METHOD OF SOLUTION

Even in those times when the chemical constitution of the fats was still unknown it had been surmised that the changes which oils and fats underwent on keeping were simply the result of oxidation. This is the view most generally held at the present time, and the more recent literature on the subject indicates that this phase of research is to be continued with no less abated interest. It is still unknown whether the development of undesirable flavors in storage butter is dependent upon an oxidation occurring in the fat itself or whether the milk sugar and nitrogenous constituents of the curd are those components of the butter most susceptible to oxidation. Approximately 10 per cent of the volume of butter is air,[6] and it is quite possible that, owing to the oxygen of the air inclosed within the material, a slight and progressive oxidation may take place in the interior of a package of butter. This possibility, when considered together with the known fact that marked and undesirable alterations in the flavor of butter during storage may be brought about by acidifying the pasteurized cream from which the butter is made,[7] has suggested the idea that an examination of the air inclosed within packages of butter differently prepared and in butter fat alone might furnish some interesting data as to whether the undesirable chemical changes occurring in stored butter are caused by a progressive oxidation in the fat itself or in some one or more of the nonfatty ingredients.

It was deemed advisable to pursue this line of investigation in a manner not previously attempted, so far as known. Samples of pasteurized sweet-cream butter, butter made from pasteurized cream to which lactic acid has been added, and butter made from pasteurized cream to which a starter had been added and which was churned at once, were prepared, packed in glass tubes, and stored. Tubes from each lot were removed from storage after certain intervals of time had elapsed and an analysis of the air therefrom was made by means of the gas apparatus specially designed for the purpose. It was hoped that the analytical data so obtained would show some distinguishing features between the three samples dissimilarly prepared, especially with respect to the sample made from acid cream. It was also decided to make use of the determination of the chemical constants of the pure butter fat to serve merely as an indication as to whether any chemical alteration of the fat through oxidation had occurred during the storage interval, confirmed by the analysis of the air extracted from packages of butter fat to determine whether the oxygen content therein is diminished during the storage period. The data so obtained were used as a standard, and the aim kept in view was to study the effect, if any, of the presence of varying amounts of nonfatty constituents (protein, lactose, etc.) upon the decomposition of the fat of butter and, in addition, to note whether the presence of varying quantities of these substances in the butter induced an alteration during storage in the composition of the air incorporated in the samples at the time of their manufacture. Samples of pure butter fat and of butter containing varying quantities of buttermilk were also prepared, packed into tubes, and stored under the same conditions as the foregoing samples. The effect of a large amount of air upon a small quantity of butter fat and upon buttermilk containing varying quantities of acid was studied by filling other tubes with pumice fragments which were then impregnated with fat or buttermilk and an analysis of the air therefrom made after certain intervals in storage had elapsed.

SUMMARY AND CONCLUSIONS

The composition of the air confined within a package of pasteurized sweet-cream butter known to contain bacteria and made from cream having an acidity of 0.11 per cent (calculated as lactic acid) showed little or no variation from its original composition after successive periods in storage, aggregating six months, at a temperature of 0° F. A small quantity of the buttermilk from butter made from pasteurized sweet cream having the same low degree of acidity as the cream above mentioned, when exposed to the influence of a very large and confined surface of air, appeared to have little, if any, effect upon the original composition of the air when the buttermilk was stored for nine months under like conditions of

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*Abstract from a paper published in the Journal of Agricultural Research, Vol. VI, No. 24, Department of Agriculture, Washington, D. C.

[1] LAXA, Otakar, and KONECNY, Alfred. 1913. Die Lipoide des Zentrifugenschlammes und ihre Bedeutung fur die Bildung des Milchfettes. *In* Milchw. Zentbl., Jahrg. 42, Heft 22, p. 663-671; Heft 23, p. 691-697.

[2] MACKEY, W. McD. 1915. Note on the behavior of some oils and fatty acids in Mackey's cloth oil tester. *In* Jour Soc. Chem. Indus., v. 34, no. 11, p. 595-597.

[3] MASTERS, Helen, and SMITH, H. L. 1914. The changes in the character of fats during the process of cooking. *In* Analyst, v. 39, no. 461, p. 347-350.

[4] PONZIO, G., and GASTALDI, C. 1912. Relazione fra il numero di iodio e la struttura degli acidi della serie oleica. *In* Gaz. Chim. Ital., v. 42, pt. 2, fasc. 1, p. 92-95.

[5] FOKINA, S. 1912. Novye izomery olsinovoi kisloty [Sur les nouveaux isomères de l'acide oléique]: $CH_3(CH_2)_6CH=CH(CH_2)_{12}COOH$ i $CH_3(CH_2)_7CH=CH(CH_2)_{11}COOH$ i vliianie pepomieshchenila dvoinykh svinzei v chastitsie. [In Russian.] *In* Zhur. Russ. Fis. Khim. Obsheh. Chast Khim. (Jour. Soc. Phys. Chim. Russe, Sect. Chim.), t. 44, vyp.-8, p. 653-661.

[6] ROGERS, L. A., BERG, W. N., and others. 1913. Factors influencing the change in flavor in storage butter. U. S. Dept. Agr. Bur. Anim. Indus. Bul. 162, 69 p., 1 fig.

[7] ———and GRAY, C. E. 1909. The influence of acidity of cream on the flavor of butter. U. S. Dept. Agr. Bur. Anim. Indus. Bul. 114, 22 p.

temperature. A portion of this same sample of sweet-cream butter when kept at a temperature of 32° F. showed a decided change in the original composition of the inclosed air, a change which was still further increased when the butter remained for a short time at room temperature. This change in the composition of the air originally incorporated into the butter was expressed by a decrease in the percentage of oxygen and a corresponding increase in the percentage of carbon dioxid. This sample of sweet-cream butter still possessed a good score after six months' storage at a temperature of 0° F., there being no indication of any undesirable flavor.

The change in the composition of the air initially inclosed within a package of butter made from sweet cream and churned immediately after the addition of 15 per cent of a commercial starter showed but little variation from that observed in the sample of sweet-cream butter when the two samples were kept under comparable conditions, both being in storage at a temperature of 0° F., although the acidity of the cream in the first case was somewhat higher (0.25 per cent) than that of the cream from which the sweet-cream butter was made. This sample of butter also displayed good keeping qualities during its storage period of nearly seven months at a temperature of 0° F.

The composition of the air inclosed within a package of butter made from sweet cream and churned immediately after the addition of lactic acid, the total acidity of the cream being about six and one-half times greater than that of the cream from which the sweet-cream butter was made, showed pronounced variations from its original composition during successive periods of storage at a temperature of 0° F. These variations were still greater when the sample was allowed to stand at a temperature of 32° F. In this case there was a considerable and a progressive decrease in the original oxygen content, as well as in the original carbon-dioxid content. A small quantity of the buttermilk from butter made from pasteurized sweet cream and churned immediately after the addition of lactic acid, when exposed to the action of a very large and confined surface of air under the same temperature conditions, showed precisely the same phenomena with respect to alteration in the original air composition. The oxygen content of the confined air had entirely disappeared within a month's time. The carbon-dioxid content, originally 2.37 per per cent had increased to more than 34 per cent within the same interval, after which time it had begun to decrease. The flavor of this butter, which was prepared from pasteurized sweet cream and churned immediately after the addition of lactic acid, was somewhat unclean after a storage period of only three months at a temperature of 0° F., and decidedly so after being in storage for six months under the same conditions.

Further, it has been indicated by the investigation pursued with pasteurized, ripened-cream butter through the successive steps from nearly pure butter fat to samples of butter containing varying quantities of ingredients other than fat and, finally, to samples containing the greatest quantity of protein, lactose, etc., and stored for a reasonable length of time (six months) at a temperature of 0° F., that the amount of carbon dioxid inclosed in a package of the material is directly proportional to the quantity of these ingredients contained therein. It has been shown that this quantity of carbon dioxid may increase during the earlier part of the storage period, followed by a decrease during the latter part. It is also of especial significance, perhaps, that the oxygen content of the gas in the material undergoes a marked and striking decrease during the interval that the samples of butter containing the varying amounts of constituents other than fat are retained in storage, and that this decrease is likewise proportional to the amount of acid and ingredients other than fat contained in the butter.

The fat of butter made from pasteurized cream, on the contrary, undergoes no apparent oxidation during the same storage period when kept at a temperature of 0° F. It is only when a substance like pumice, which may have catalytic properties, is impregnated with a small amount of butter fat and exposed to the action of a large amount of air while kept at a temperature of 32° F. that a very slight oxidation is noticeable.

The results of the investigations may be summed up as follows:

(1) The development of undesirable flavors in butter held in cold storage at a temperature of 0° F. is not dependent upon an oxidation of the fat itself.

(2) The production of "off flavors" so commonly met with in cold-storage butter is attributable to a chemical change expressed through a slow oxidation progressing in some one or more of the nonfatty substances occurring in the buttermilk.

(3) The extent of this chemical change is directly proportional to the quantity of acid present in the cream from which the butter was prepared.

(4) The quantity of carbon dioxid present in cold-storage butter appears to have a certain relation to the quantity of buttermilk in the butter. During storage this quantity of carbon dioxid may increase to a maximum followed by a progressive decrease.

Soap Bubbles of Long Duration

THE season of the Royal Institution evening discourses was opened by Sir James Dewar, F.R.S., Fullerian Professor of Chemistry, who delivered a lecture on "Soap Bubbles of Long Duration." Longevity, he remarked, could only be demonstrated by registers, and he pointed to the bulky notes in which his observational records were entered. Last year he had dealt with the finest soap films, which, when produced in an atmosphere containing nothing but water vapour—no air—could easily be preserved for many months. The large glass bottle containing the horizontal films shown in January, 1916, was on the lecture table; the "black" film, only visible in reflected light, was still intact. The study of soap bubbles introduced further difficult problems; there were the difficulties of internal gas pressure and of the transference of gas from the inside of the bubble, in addition to the primary difficulty of isolating one particular bubble for the examination to which he had submitted films. Why had he taken up these studies, Sir James asked. Years ago he had lectured on soap bubbles, and he had then been fascinated by the wonderful work of Joseph A. F. Plateau, of Brussels, Professor of Physics at Ghent, who had entered upon the investigation of soap bubbles in 1842, when 40 years old, and practically blind owing to his experiments on the action of intense light on the retina. Plateau was also a painter, and that had helped him to continue his researches with the aid of assistants till he died, when nearly 80 years of age.

When soap bubbles were blown in air the air should be free of dust. The part played by the impurities of the air in putrefaction had been discovered by Pasteur, and Tyndall had later demonstrated the gross impurity of the air and its purification in very valuable ways. The lecturer concentrated the beam of an arc lamp by a lens; a whitish cone of light was seen in the impure air of the lecture theatre, but when the many organic particles in that dust were burnt by placing a burner under the beam, peculiar black shadows of "optically pure" air were noticed moving in the beam. Tyndall had further purified the air in glass boxes by smearing the glass with glycerin; in Tyndall's box exhibited, into which air was admitted through cotton-wool filters, no putrefaction had taken place, and the Tyndall effect test for dust-free air—invisibility of a strong beam of light—was well known. He further demonstrated it by some novel experiments. He sent a beam of light upward through a glass bottle containing ordinary air; when a blast of air which had been strongly compressed (for subsequent condensation and liquefaction), and thus been heated, was passed through the bottle, the beam became much fainter. He also placed the electrode of a Wimshurst machine axially in the beam, the bottle in this case having been filled with smoke from some burnt bituminous matter; after the machine had been worked for a few minutes the air clarifier and shadows of black were seen to develop in the beam. Illuminated by the arc, the space inside a big soap bubble, blown in pure air, within a glass case, looked dark. Ordinary London air contained about 100,000 particles of organic and inorganic matter, often greasy, per cubic centimetre. A speck of grease on a soap bubble would, of course, affect the surface tension of the bubble and make it break down. It was astonishing that people should have blown bubbles for 50 years after Plateau, in the open air, without suspecting the air; but he had not particularly referred to this point last year because he had not known it himself then.

As regards the soap solution, Plateau had used a 2 per cent solution of olive-oil soap containing 30 per cent of glycerin. For his own experiments, in which he measured and weighed and tested, he sometimes used high percentages of glycerin, and he preferred the purest oleic acid (tested by the iodine number) and ammonium (not potassium or sodium) soap, because there was a very sensitive reagent for ammonia (Nessler's—mercuric iodide gives, with a trace of ammonia, a reddish-brown precipitate or color, as demonstrated), with the aid of which the disappearance of ammonia from the film could be followed. The bubbles were blown in purified air, by opening a stop-cock in the air supply tube, or with the mouth. Liquid air might be used, of course, but it would have to be heated up first; ordinary air used for blowing was filtered through cotton wool; several devices for this purpose were shown. Bubbles could be blown hanging or resting (upon a wide ring, for example). The little bubble or sac of liquid at the bottom of a bubble could be removed by suction through tubes applied from outside (the tubes used in blowing), or tubes passed from above axially into the bubble. Properly drained, the bubbles lasted a long time; but bubbles were more sensitive to tremors than films. Some of the bubbles exhibited were nearly ½ m. in diameter; they were blown in cubical or oval glass vessels containing pure air at atmospheric pressure. Some water was always kept in the bottom of the containing vessel.

The experiments which Sir James then briefly outlined with the aid of slides concerned the life of bubbles, the changes in the diameter of the bubble (spontaneous contraction) and in the liquid forming it, and the transference of gas through the skin of the bubble from the inside to the outside of the bubble. His first bubble by the improved processes had only lived three days, but the life had soon been prolonged; a bubble 20 cm. in diameter had held out 95 days, one of 40 diameters 63 days, smaller bubbles nearly a year. A uniform cool temperature (about 10 deg. C.) favored longevity. The freshly blown bubble, we might perhaps interpose, shows, of course, an irregular streaming of the liquid in the film; as the excess of liquid drains off the visible motion ceases, the bubble thins out and becomes black in the upper portion, while colored rings appear near the bottom, in which streaming may still be observed. At the same time the "condensation" sets in, water vapor was absorbed by the glycerin of the bubble; the bubble solution was thereby diluted, but the film still remained thick enough to display colors. This condensation was watched by periodic analyses of the drops falling from the bubble, which were collected in a graduated tube leading out from the containing vessel. The condensation amounted, for example, to 0.412 milligramme on the first day and, decreasing rapidly, to 0.003 milligramme on the 20th day, the weights named being per square centimetre per day. Only 2 or 3 per cent of the glycerin originally in the solution might be left in a fortnight, and in this way 90 per cent of the soap had been washed out in 20 days.

Owing to the surface tension in the film there was further contraction of the bubble, accompanied by the transference of gas through the film outward. The rate of the contraction and transference varied with the gas; it was much more rapid with hydrogen than with air; a hydrogen bubble would be blown in hydrogen at atmospheric pressure. The contraction of the diameter increased more rapidly as the time advanced and the diameter became smaller, the curve, diameter plotted against time, being parabolic. The internal pressure varied inversely as the diameter. In a diagram exhibited the diameter of a bubble diminished from the value 1 to 0.2 in 24 days; in the case of a black bubble the diameter had been reduced from 38 to 1.5 cm. in five weeks. Black bubbles best showed this contraction, which depended upon the thinness of the film and the dilution of the solution; but it was very well observable also in colored bubbles. The gas transference amounted to 0.023 cub. cm. per square centimetre per day in one case quoted. Finally, the speaker exhibited a series of colored photographs which brought out well the gradual color changes and the beautiful colors displayed; in some of these photographs the sharp boundary lines of the black zone and of the silvery zone and the colored rings were quite distinct; others exemplified more the gradual transition of pink into green, etc. Peculiar color effects were seen in the bubbles and in these photographs when the illumination was by mercury-vapor lamps.

Detection of Artificial Coloring Matters in Wine

DARK red wines, even when diluted with five parts of water, sometimes dye wool so intensely that the color cannot be removed by washing with boiling water; this may lead to the conclusion that an aniline dye is present in the wine. If, however, the wine is diluted previously with 10 part of water, the color is usually removed from the wool when this is washed with boiling water. In doubtful cases the colored wool should be heated on a water-bath with 1 per cent ammonia; this destroys the natural coloring matter derived from the wine, while aniline dyes, for the most part, go into solution. The ammoniacal solution is then acidified with sulphuric acid and heated with a fresh thread of wool. If aniline dyes are present the wool is colored, but in their absence it remains white.—H. KREIS in "Schweg. Verein Anal. Chem." From a note in the *Journal of the Soc. of Chemical Industry*.

The Value of High-Level Meteorological Data—II*

In Forecasting Changes of Temperature

By S. P. Fergusson[1]

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ANALYSIS OF RECORDS FROM MOUNT ROSE AND VALLEY STATIONS

In studying the connection between the conditions on Mount Rose and the occurrence of frost in the valleys, the data from the upper and lower stations have been analyzed according to the following general plan:

(1) Simultaneous records of pressure, temperature, humidity, and wind at the upper and lower stations have been plotted together, to show the general character of changes of condition from day to day.

Two of these plots are reproduced in Figs. 5 and 6.

At first it was assumed that facsimile copies of the original record sheets would serve the two purposes of indicating the character of the records and the more obvious correlations between data from different elevations. A few plots of this kind were made, but the work proved to be very slow and tedious, for the reasons that errors could not be easily corrected, direct plots of some traces could not be made, and that there are practical limits to the number of tracings that can be plotted on diagrams with curved coördinates such as are employed as record sheets. During the summer vacation the writer constructed a plotting machine which in operation differs from the ordinary pantagraph in that either dimension of a diagram may be changed without altering the other, and that curved coördinates can be changed into rectilinear. With this machine numerous plots have been made in which all records from the three stations are represented on rectilinear coördinate paper in one diagram, in which scale and time errors are automatically corrected, and the traces are in drawing ink ready for the engraver without going through an intermediate process of "dotting in" the outline of the curve or copying it in pencil and inking over. These plots are much more accurate than those made in the usual way, and it is considered unnecessary here to reproduce the original records in curved coördinates, since there is no difference between these original records and the copies such as are represented in Figs. 5 and 6, except that the latter are in rectilinear coördinates and the errors have been corrected. The plots of tabulated data from Blue Hill also have been made in one operation by means of the machine. The writer believes that, in addition to the gain in accuracy by the use of this machine, fully three-fourths of the time usually required in plotting can be saved.

(2) The abnormal falls of temperature occurring on Mount Rose between 1911 and 1914, inclusive, were tabulated together with the local weather conditions prevailing at the time and compared with the conditions existing simultaneously at the valley stations and with the position of the nearest cyclone and anticyclone; also, the relation of the conditions prevailing at the times of these cold waves, to four-year normals has been determined. These normals are averages of bihourly values of pressure, temperature, and wind-velocity for January, April, July, and October, from 1911 to 1914, at both upper and lower stations, and are given in Table II.

(1) As in the instances of data from Pike's Peak and other stations shown in Fig. 2, the plots of synchronous records from Mount Rose and valley stations serve to show the more obvious characteristics of phenomena at different heights. The examples given represent but a small part of the time for which data are available, but other comparisons made at random show conclusively that the same general relation or connection between the records at high and low levels is a permanent one. The plots are practically self-explanatory, the most noticeable facts being that changes of temperature in all seem to occur at nearly the same time at all levels. This is more evident if the tracings are smoothed and the diurnal variations eliminated.

(2) From the table of abnormal falls of temperature at the summit was ascertained the number of times these cold-waves were followed by high, low, or normal minimum temperatures at the valley stations; these classified minima were compared with the prevailing weather conditions to determine their relation to the state of the sky, whether clear or cloudy. Of 32 abnormal falls of temperature observed, 10 were followed by low minima at the valley stations within 48 hours; 7 were followed by higher minima than occurred on the preceding days, and in 15 cases there were no important changes. Comparisons of these minima with the weather conditions show that the abnormally low minima at the base stations almost without exception occur during clear weather, particularly when the sky is clearing after a general storm, and frequently when no important changes of temperature occur on the summit.

In comparing the abnormal falls of temperature with reference to the distribution of pressure (or the positions of cyclones and anticyclones) the former were divided into three groups: those followed by lower minima at the base stations, those followed by higher or stationary minima, and those where the minima at summit and base occurred at the same time. The positions of the cyclone and anticyclone were determined for the middle of the cold wave. In the first two groups the distribution of pressure was practically the same—a cyclone 500 miles south or southeast of Mount Rose and an anticyclone 300 to 500 miles to the west-northwest. In the third group there is a more definite connection. In practically all the cases examined where the minima occurred simultaneously at the summit and base, a cyclone or anticyclone was passing very near, if not over, Mount Rose. This is a result to be expected, since, when well-defined cyclones or anticyclones move over a given region, the winds are relatively strong at all levels and the atmosphere more nearly homogeneous.

The classification of the data from Mount Rose with reference to the distribution of pressure and storm-movement is only roughly approximate, since in this region cyclones and anticyclones are seldom well defined, their movements are irregular, and the horizontal gradients of pressure and temperature are small. A well-marked characteristic of the weather in this region is the persistence, for long periods of time, of periods of fair weather or storm, due to the passage of a succession of small cyclones or anticyclones. Frequently a storm with attendant phenomena of cloudiness, daily showers and relatively strong winds will continue for week or ten days and be followed by a period of fair weather of even longer duration. The sequence of conditions has led to the popular saying: "We get our

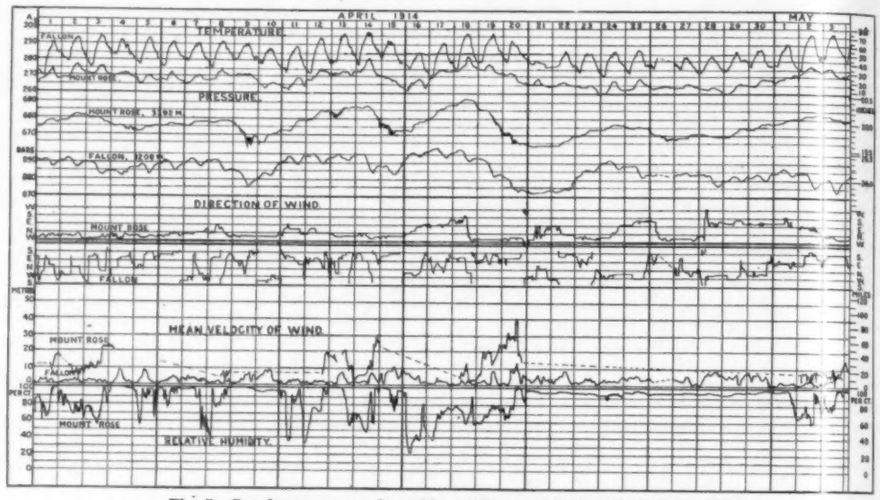

Fig. 5—Synchronous records at Mount Rose and Fallon, April–May, 1914

TABLE II

Average pressure, temperature, and velocity of the wind at Mount Rose and Fallon for the four years, 1911 to 1914

Mean Pressure, Mount Rose, 3,292 meters—In Bars

Month	2 A.M.	4	6	8	10	12	2 P.M.	4	6	8	10	12	Daily mean
January	677.4	677.4	677.6	678.0	678.0	678.3	678.2	678.2	678.1	677.7	678.0	678.0	677.9
April	675.9	675.5	675.6	675.4	675.8	675.9	677.7	676.2	676.0	675.8	674.7	675.9	675.9
July	688.3	688.3	688.3	688.6	689.1	689.3	689.6	689.3	689.1	688.9	688.9	688.7	688.9
October	682.6	682.6	682.6	683.0	683.3	683.3	683.5	683.5	683.4	683.4	683.3	683.0	683.1
Year { Bars	681.1	680.9	681.0	681.2	681.5	681.7	682.2	681.8	681.6	681.4	681.2	681.4	681.4
Year { Inches	20.11	20.11	20.11	20.12	20.13	20.13	20.15	20.14	20.13	20.12	20.12	20.12	20.13

Mean Pressure, Fallon, 1,208 meters—In Bars

January	883.8	883.5	883.4	883.9	884.8	883.9	883.5	883.5	883.8	884.0	884.2	884.3	883.8
April	881.3	881.3	881.9	882.6	881.9	881.1	879.3	878.6	878.9	879.8	880.6	881.0	880.7
July	892.6	893.1	893.8	892.5	891.7	879.7	877.1	875.5	876.3	878.7	881.0	881.6	880.3
October	886.1	886.4	887.0	887.7	886.4	886.2	883.9	883.7	884.2	885.2	885.9	886.1	886.6
Year { Bars	883.4	883.6	884.0	884.1	883.7	882.5	880.9	880.3	880.4	881.9	882.9	883.2	882.6
Year { Inches	26.09	26.10	26.11	26.11	26.10	26.06	26.01	26.00	26.00	26.04	26.07	26.08	26.06

Mean Temperature, Fallon—In Degrees Absolute

January	272.8	272.8	272.5	272.9	276.3	278.3	279.7	279.0	276.3	274.8	273.9	273.0	275.2
April	277.2	275.9	275.3	281.6	284.6	288.0	286.2	286.9	286.2	282.8	280.6	279.0	282.4
July	288.1	286.6	288.4	295.1	299.2	301.3	303.3	303.2	301.1	295.8	293.0	290.4	296.5
October	277.5	276.1	275.5	281.5	286.4	289.6	290.9	289.2	285.1	281.7	279.9	278.6	282.7
Year { A°	278.9	277.8	277.9	282.8	286.6	289.3	290.8	290.1	287.1	283.8	281.8	280.2	283.9
Year { F°	42.6	40.6	40.8	49.6	56.5	61.3	64.0	62.8	57.4	51.4	47.8	45.0	51.6

Mean Temperature, Mount Rose—In Degrees Absolute

January	263.9	262.9	262.8	262.8	263.1	263.6	264.0	264.3	263.9	263.6	263.4	263.1	263.4
April	266.2	266.1	266.2	266.8	267.9	268.9	269.5	269.3	268.0	267.1	266.6	266.6	267.4
July	279.0	279.9	279.4	280.6	282.5	283.9	284.5	283.4	281.7	280.2	279.6	279.5	281.1
October	273.0	272.9	273.1	273.3	275.5	277.0	277.6	276.7	274.8	273.7	273.4	273.0	274.5
Year { A°	270.3	270.2	270.1	270.9	272.2	273.3	273.9	273.4	272.1	271.1	270.7	270.5	271.6
Year { F°	27.1	27.0	26.8	28.2	30.6	30.7	33.6	32.7	30.4	28.6	27.9	27.5	29.5

Mean Velocity of the Wind at Mount Rose—In Meters per Second

	2 A.M.	4	6	8	10	12	2 P.M.	4	6	8	10	12	Daily mean
January													
April													
July	9.0	9.6	9.6	8.4	8.2	7.6	8.0	9.3	10.0	8.8	8.9	8.7	8.8
October	9.8	10.1	9.6	9.1	8.3	8.4	7.9	8.2	8.2	8.9	10.0	10.2	9.1

Mean Velocity of the Wind at Fallon—In Meters per Second

January	3.0	2.4	2.7	3.0	4.2	4.5	4.5	3.9	3.7	2.7	2.7	2.9	3.3
April	1.8	1.7	1.6	1.9	2.7	3.9	4.0	4.5	4.2	3.1	2.5	2.0	2.8
July	1.5	1.4	1.1	1.6	1.9	2.2	2.9	3.3	3.2	3.0	2.5	1.9	2.2
October	1.3	1.1	1.2	1.3	2.1	2.5	2.7	2.6	2.0	1.8	1.7	1.3	1.8
Year { Meters per second	1.9	1.6	1.6	1.9	2.7	3.3	3.5	3.6	3.3	2.8	2.3	2.0	2.5
Year { Miles an hour	4.3	3.6	3.6	4.3	6.0	7.4	7.8	8.1	7.4	5.8	5.1	4.5	5.6

*From Bulletin 83 of the University of Nevada Agricultural Experiment Station.

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weather in chunks." When such a sequence of conditions is well established, a fairly high percentage of success can be attained in forecasting from day today. Most of the failures occur when the conditions are changing from stormy to fair and *vice versa*, and are due to the fact that these periods of weather vary considerably in length.

COMPARISONS WITH RESULTS OBTAINED ELSEWHERE

An examination of the results obtained at Pike's Peak and other places shows that the same general connection or relation between conditions at the summit and base of a mountain exists at every place where records have been obtained. The chief differences are due to differences in the intensity of cyclonic phenomena. Thus, at Mount Washington there is usually only a small difference in the time of occurrence of temperature changes at the summit and base, while at Mount Rose and Pike's Peak the differences are quite variable—smallest when cyclones pass near the region and largest when there are no important contrasts of pressure in the vicinity.

No comparison in detail is given of temperatures at the summit and base of Mount Royal, for the reason that the average differences published indicate unusual conditions of exposure for the differential recorder.

The results from Blue Hill, shown in Fig. 2, tend to confirm those obtained at Mount Royal to the extent that, apparently, a slightly larger number of changes of temperature at the summit precede changes in the same direction in the valley than is the case at Mount Rose or other higher level stations. As is the case elsewhere, at least one-half of the oscillations of temperature at the summit and base of Blue Hill are synchronous.

The difference of elevation between these mountain and base stations are: Mount Washington and Burlington, 1,829 meters; Pike's Peak and Colorado Springs, 2,438 meters; Mount Rose and Fallon, 2,084 meters. These are not large enough to cause important differences in the meteorological conditions, and such as occur are undoubtedly due to differences in absolute height above sea-level, local topography, and in the conditions controlling phenomena in the particular region.

The records obtained at great heights by means of kites and *ballons-sondes*, given in Figs. 3 and 4, show that changes of temperature and pressure occur progressively later with increase of height above the earth and that the sea-level type of condition usually disappears at a height of 3,000 meters. Hence, the region of the atmosphere, where data at different heights are most useful in local forecasting of temperature, evidently does not extend higher than 3,000 meters above sea-level, and very probably the most satisfactory results are to be obtained when the differences of elevation between stations is less than 1,000 meters.

EFFECTS OF LOCAL TOPOGRAPHY AND ERRORS DUE TO EXPOSURE OF INSTRUMENTS

When the first mountain observatories were established, it was assumed by many that the meteorological records obtained represented free-atmosphere conditions: but when free-atmosphere data from kites and balloons began to accumulate it was found that there are important differences in records obtained by the two methods, some of which persist through all conditions of weather, and all of which are due to the influence of the mountain itself upon the conditions in its immediate neighborhood.

In 1905, 1906, and 1907, the writer obtained a number of comparisons of records of temperature, humidity, and wind-velocity on the summit of Mount Washington with similar records from instruments carried by kites to the same elevation in the free atmosphere. Some of the results are given in the accompanying table; also in Fig. 7 are plotted the conditions in the free atmosphere, on the mountain and in the valley, showing the diurnal changes on September 6, 1906. Similar differences of temperature have been observed elsewhere, but the direct comparison of wind-velocities given is apparently the only one that has been made, although the results confirm a statement, made by Ferrel in 1885, that "the velocity of the wind in passing over a ridge or mountain range may be much greater at the top, even near the surface, than at the same level generally at a distance from it, for there is a heaping up of the atmosphere on the windward side and an increased gradient where the air passes over, for, when there is a diminished sectional area for the atmosphere in passing over the earth's surface, there must be an increased velocity."

As a part of this investigation, a similar series of kite ascensions, to determine the influence of Mount Rose upon the conditions in its vicinity, was undertaken in 1912. Several ascensions were obtained, but it was found necessary to discontinue the work until a suitable place could be found for it. The greatest elevation reached, on October 3, 1912, was within about 600 meters of the summit of Mount Rose. One very unusual condition shown by the records was that during

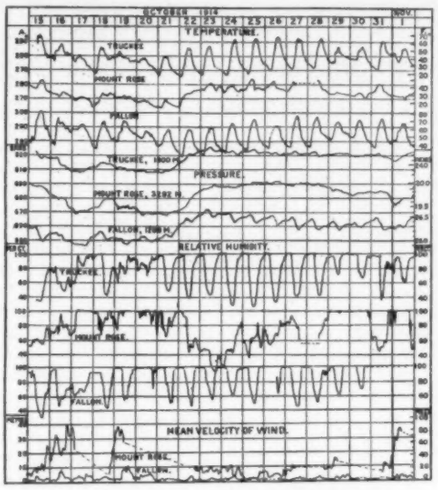

Fig. 6—Synchronous records at Mount Rose, Truckee and Fallon, October, 1914

every ascension there was a decrease of wind as the kites rose, which determined the elevation that could be attained. It could not be determined from cloud observations or the records from Mount Rose at the time whether this condition was replaced at a greater height

TABLE III

Comparison of Temperature and Velocity of the Wind on Mount Washington (1,916 meters) and in the Free Atmosphere at the Same Height

Date	Temperature A			Velocity of wind—Meters per second		
	Mount Washington	Free Atmosphere	Difference	Mount Washington	Free Atmosphere	Difference
Aug. 24, 1905	279°.1	281°.3	−2°.2	10	14	+2
Sept. 3, 1906	278.9	278.3	+0.6			
Sept. 4, 1906	280.0	277.5	−2.5			
Sept. 5, 1906	281.3	280.3	−1.0			
Sept. 7, 1906	280.0	279.3	+0.7			
July 15, 1907	286.3	286.2	+0.1	12	10	+2
July 21, 1907	279.2	279.7	−0.5	10	11	+2
July 26, 1907	280.3	280.1	+1.2	21	15	+7
July 27, 1907				13	109	+9
July 30, 1907	279.9	279.9	−0.1	18	11	+7

*Extrapolated from a record extending to a height of 1,100 meters.

by an increase of velocity with increase of height or is a peculiarity of certain conditions existing in the mountain valleys. From occasional observations of clouds, it is evident that the increase of velocity with elevation found elsewhere also prevails in this region, although the movements of clouds appear to be rather more variable than is the case on the Atlantic Coast. Hence, until more data are available, it seems probable that all these ascensions, by chance, were made during the prevalence of the same weather conditions.

During midday calms on Mount Rose the air in contact with the southern slopes is heated considerably above the true air-temperature at the same elevation, and affects the records of the meteorograph on the

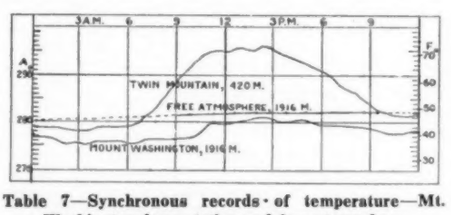

Table 7—Synchronous records of temperature—Mt. Washington, base station and free atmosphere

summit in much the same way that instruments at lower levels are affected by heated air near the ground. The writer has observed instances when, after a calm lasting several hours, a sudden breeze caused a fall of temperature of 5° or more. When there is wind the air driven mechanically upward and over a mountain is cooled as it expands, and if, at the time, the free-atmosphere gradient is smaller than the adiabatic for dry air, the air-temperature recorded on the summit will be lower than that of the free atmosphere at the same height. Hence, variations of temperature amounting to nearly one-half the normal diurnal range may be due to the mountain itself and have very small relation to the prevailing weather conditions. At the present time the data are insufficient to determine whether the influence of mountains upon weather conditions is in proportion to their height or mass, although it is very probable that the records obtained on peaks with very steep sides are in closest agreement with free-atmosphere conditions.

Concerning the exposure of the instruments, it is only necessary to state that the meteorographs and standard thermometers with which they are compared are in standard shelters, the errors of which, due to lack of ventilation, etc., are small. Comparisons with an Assmann aspiration psychrometer on clear, calm days show that the temperature within the shelter is seldom more than 1° A. too high at such times. Also, on clear days the side of the shelter on which the sun's rays fall is, for the time, about 1° A. warmer than the opposite side. At Fallon, in a shelter resting on the ground, the maximum temperatures usually are 0.5° to 1° higher and the minimum temperatures 0° to 2° lower than those recorded in the meteorograph shelter, which is 1.8 meters above the ground.

The results from Blue Hill show several instances of abnormal gradients, of which that of July 29th is perhaps the most conspicuous. At this time the wind at the summit was very light, for several hours averaging less than 3 meters per second, or less than half the normal. When these conditions prevail, abnormal gradients could be caused by a difference in the direction of the wind at the upper and lower stations or by errors of the thermometer shelters due to a lack of ventilation, which exists during calms. That even within this small difference of elevation (180 meters) differences in the direction of the wind are important, is shown by the fact that of a large number of inversions of temperature at Blue Hill studied by the writer in 1888, about one-tenth occurred on cloudy nights when a slowly moving northerly current prevailed in the valleys beneath a warmer southerly wind at the height of the summit.[2]

SUMMARY

The general relation or connection between the conditions recorded at the summit and base stations of Mount Rose appears to be practically the same as that found to exist between the summit and base stations of mountains in other parts of the world.

Of the decided falls of temperature or cold-waves occurring on the summit during four years of observation, about one-half were accompanied by nearly synchronous changes at the base stations; one-third were followed within forty-eight hours by lower minimum temperatures at the base stations; one-fifth were followed by a slight rise of temperature at the base stations.

In the instances where cold-waves on the summit precede those at the base, particularly those where a rise of temperature occurs at the base, the cause is probably local gradients less steep than usual, mechanical cooling of the air at the summit during a strong wind, or clouds or fog in the valleys and below the summit. Such a condition, however, does not appear to be a very stable one and probably cannot exist very long.

Abnormal falls of temperature or cold waves occur most frequently when a cyclone or area of low pressure is about 500 miles south or southeast, and an anticyclone or area of high pressure about 300 miles northwest of Mount Rose.

When well-defined cyclones and anticyclones pass over or near Mount Rose, the changes of temperature at the summit and base are nearly synchronous, for at such a time the winds at all levels are higher than normal and the atmosphere more nearly homogeneous.

It is believed that data from high-level stations will be found valuable in local forecasting when studied with reference to the prevailing meteorological conditions, as shown by the daily weather maps. However, since the processes of the free atmosphere are not as yet fully understood, particularly in this region where no systematic aerological exploration has been made, it will be necessary first to determine the vertical gradients or distribution of the chief meteorological elements by means of recording instruments elevated by kites and balloons and from observations of the formation and movements of clouds. This work should be done in some level region, such as the Carson Sink, where the phenomena of the free atmosphere are not influenced by neighboring mountains or valleys. Comparisons of free-atmosphere data with observations on mountains and in valleys under various conditions of weather will show the relation of local phenomena to the general movements of the atmosphere.

Practical use of the results of an investigation of this kind can be made by embodying the information in courses of study, and in publications, so that, in time, the residents of any community familiar with local conditions and having access to the daily weather maps will be able to make local forecasts more accurate than those based upon local or general data alone.

The writer believes that the local weather maps could be improved by the use of data from a large number of stations and by reducing the data to the average level of the region as well as to sea-level, for thereby where changes of pressure are small the effects of errors of reduction will be lessened. Further improvement could be effected by adopting the plan of the International maps wherein the pressures are published in C. G. S. units, so that the pressure at any level is a direct percentage of the entire standard atmosphere near sea-level.

[2]Annals of Harvard Observatory, Vol. XX, Part I, Appendix C, 1889.

Phosphorescence*

And the Phototropy of Inorganic Systems

By José Rodriguez Mourelo, Professor in the Industrial School, Member of the Royal Academy of Science of Madrid

SINCE 1896 I have devoted a large part of my time to the study of the production of the phenomena of phosphorescence and of phototropy in inorganic bodies and systems, whose conditions of formation I have varied as much as possible, as well as the methods of preparation and even the constitution. I have essayed to determine the diverse actions of light upon bodies and their relation with the different manners in which the systems are formed, with the purpose of determining the precise part played by chemical reactions in photoluminescence and phototropy, and of elucidating the peculiar mechanism of photochemical actions. But I must admit that I am not yet ready to formulate a doctrine, nor even to suggest a general law of phenomena.

Light acts upon certain bodies in various ways; causing a change of color while its action endures, with a return later to the original coloration, or making them luminous for a brief instant, or rendering them phosphorescent for a certain length of time, thus giving rise to the phenomena of *phototropy, fluorescence, and luminescence.*

Each of these phenomena has its essential characteristics; but there are numerous varieties in each category which relate them one to the other. Considering the resemblances and points of contact between these three phenomena we have regarded them as distinct manifestations of a single fundamental fact, which depends rather upon the state of molecular aggregation than upon the nature of the substances which present it. It is in this sense that certain physicists admit that fluorescence is proper to the liquid state, and phosphorescence to the solid state. Moreover, facts are not lacking which induce us to admit an entire individuality for each of the three phenomena, in spite of the analogy or even coexistence which they at times present in the same body.

I have never pretended to resolve the problem of the causes of the molecular modifications which we observe in changes of color, or in a special emission of light; still less have I sought to establish a certain theory of photoluminescence, of chemical nature. However, after my experiments I have not been able to refrain either from the quantitative examination of facts or from certain theoretic conceptions, which have led me to announce a certain law for phototropy analogous to the law called the optimum for phosphoresence. I have sought, in the first place, to obtain numerous series of phosphorescent bodies of the most varied materials, but whose basis has always been the sulphides of barium, of strontium, and of calcium. It is in this way that I have discovered the phototropy of inorganic systems that I have recently studied. Subsequently, I have endeavored to determine the differences of impressionability by light and of colorations, in their relations to the nature of the substances considered as active in each system, the duration of the phenomenon, its persistence, and most particularly the influence of the methods and of the circumstances of obtaining the systems, above all of the temperature.

Later I attempted to deduce from the *ensemble* of the facts observed, the structure of phosphorescent substances, and the relation of their components, constantly considering phosphorescence as a molecular phenomenon. Still more ambitiously, I have tried to explain the emission of cold light, by attributing it to a reversible change, absolutely chemical in nature. Such theoretic temerities have only the value of approximations; but, based upon facts directly observed, they are not without some degree of foundation.

Not only have they led to new and more searching investigations, but they have been of further value when I undertook to distinguish the group of characteristics common to all phosphorescent matter and to which phosphorescence is to be attributed. Foremost of these is a white or grayish color effect.

In all my experiments I have employed as luminescent substances the alkaline-earth monosulphides of barium, strontium, or calcium, but never pure, since in that state they are not sensitive to the actions of light, nor impressionable by any of its radiations. In reality, the phosphorescent substance is not a sulphide, but the aggregate, often complex, which contains it, and which must be formed in a special manner, at a high temperature, and under special conditions, upon which, apparently, depends its sensibility, more or less considerable, to the actions of light. For example, early in my researches I attempted to obtain phosphorescent sulphide of barium by reducing impure sulphate of barium by carbon. I had taken all sorts of precautions, but the product, of a slightly grayish tint, was absolutely insensible to light. I heated it anew to a higher temperature, nearly 1000° C., and then obtained a sensitive product, endowed with an intense yellow phosphorescence. The influence of temperature sometimes permits the changing of the color of the phosphorescence; I have thus obtained calcium sulphide with violet, green, or yellow phosphorescence, and green or yellow strontium sulphide, in the same experimental conditions. I made numerous efforts to establish the experimental law of such variations, but without arriving at a definite conclusion; and I had no better success in seeking to establish a relation between the temperature of formation of the sulphides and the color and intensity of their phosphorescence. I can only state that with the same methods of procedure the calcium sulphide became phosphorescent at a lower temperature than the strontium sulphide, and that barium sulphide required a higher temperature than either of the others. But many times I heated these singular bodies, desiring to obtain them in a state of greater activity, yet they became inert and insensitive, despite all precautions to prevent excessive oxidations, or other transformations of the system formed. On the other hand it sufficed, at times, to heat sulphides which had lost their phosphorescence, for no matter what reason, in inert atmospheres, and, if oxidation was not very much advanced, they often recovered their sensibility to light.

The only unfailing rule which can be established with any degree of generality for the preparation of phosphorescent sulphides is that in every case there must be present a small quantity of certain compounds of the heavy metals; and that these must be distributed with the greatest possible uniformity through the reagents employed to form the sulphides.

I have employed the reduction by carbon of the corresponding sulphates with very serious results. I have essayed to treat highly heated oxides with vapor of sulphur or with very dry sulphur (sulphuretted hydrogen) gas, and to decompose by heat the sulphites and the hyposulphites; I have tried the action of flowers of sulphur on carbonates at a high temperature. In all these cases it was necessary to add to the primary substances very small proportions of metallic substances, often not exceeding a few tenths of a milligram.

In the numerous series of my experiments I have obtained the best effects with the simplest systems—hence I do not comprehend the necessity of mingling substances so varied (as some investigators have done) when with none of these complications one can obtain bodies endowed with the highest phosphorescent sensibility. Thus I may mention a sulphide of calcium prepared 14 years ago; it is so sensitive to light that it suffices to expose it to its influence for one second, without isolation, to cause it to become brilliant in the dark, with the most intense phosphorescence, of a deep violet color, for nearly an hour; and despite the lapse of time since its preparation it retains this notable property without the slighest alteration. I obtained it with absolutely pure calcium carbonate, without the slightest trace of iron, by adding a single milligram of silica per hundred grams of carbonate, with the proper proportion of flowers of sulphur, and heating in a covered crucible to 900° C., for 4 hours continuously, and then cooling slowly.

After many trials I have succeeded in establishing an efficacious method for the admixture of what I call, for the moment, impurities, but which are, as we shall see, substances indispensable for the production of phosphorescence, with the primary material of the sulphide, which has been, generally, the corresponding carbonate, precipitated, very white, pure and dry. The method consists in employing very dilute solutions of these substances in water or alcohol: with these dilutions the whole quantity of the carbonate is impregnated; it is then dried on a water-bath while being constantly stirred. It is then mixed with the estimated quantity of flowers of sulphur and heated in an earthen or porcelain crucible after being covered with a layer of starch in a finely powdered condition. Experiment has shown me the utility of the presence of very small proportions of alkaline substances, particularly of the carbonate and the chloride of sodium in the mass of the reaction, which may be added at the moment of the impregnation of the carbonate by the metallic substance. I do not consider the alkaline substances indispensable, but merely convenient; their influence with regard to augmentation of the sensibility of the systems is very manifest, at least in certain cases.

Among the other personal observations relating to the preparation of the phosphorescent sulphides of barium, strontium and calcium, I may indicate, first of all, the absolute necessity that the sulphur should be white, or at least a very pale gray. If the proportion of sulphur employed has been excessive, then poly-sulphides are easily formed, especially with barium. This, of a peculiar greenish tint, is distributed throughout the whole system, rendering it completely insensible to all categories of luminous rays. The same thing occurs if the compounds utilized as primary substances contain iron, or if an excess of a heavy metal be employed as an impurity, the sulphide being black or very dark. It is important to insist upon these points. In my opinion the alkaline earthy sulphide is sensitive to the light and phosporescent because it is white, as is also the sulphide of zinc, whether amorphous or crystallized, as, for example sidot blend, which is phosphorescent and very sensitive to light. The white color of the sulphides of barium, strontium and cadmium is connected with their great purity, and conditioned in them by a certain transparence to radiations, without which their impressionability with respect to light would not exist. We have a direct proof of this in the fact that when a very small quantity of iron chloride—about a tenth of a milligram—is added to a very pure strontium carbonate, in order to prepare the corresponding sulphide, the latter is neither white nor phosphorescent, while it is both if the iron be excluded. In the same way, if we act so as to produce the poly-sulphides, or even if we employ more than half a centigram (per hundred grams of carbonates) of impurities such as bismuth and copper, in the form of chlorides or nitrates, the resulting products are neither white nor phosphorescent, but masses inert to the direct and continued influence of light. From these experiments, oft repeated, we must deduce the necessity of operating with masses which are either white, or very pale in tint, to achieve the best results.

Becquerel and Vermeuil made experiments which are classic with regard to the quantity of the foreign substance required to obtain the maximum of sensibility to light, and of intensity and duration of phosphorescence. My own tests have brought me to the same conclusions as those of Urbain and Bruninghaus, which are the foundation of the law called "the optimum."

Employing invariably the same method to obtain phosphorescent sulphides, and in testing, in the rôle of impurities, the most varied metallic substances, in the form of chlorides, nitrates, or sulphates, I have observed that there exists a certain relation between the proportions of the impurity on the one hand, and, on the other, the maximum sensibility of the products, and the greatest intensity and duration of the phosphorescence; the optimum of these two qualities is obtained with a certain proportion of these substances, and the result is altered if this proportion be made greater or less.

To demonstrate the relative degrees of sensibility of different metallic salts I operated as follows: I mixed variable quantities of each of them, from a centigram to a tenth of a milligram, each time with 100 grams of precipitated strontium carbonate, according to the method of impregnation described above. Flowers of sulphur was added, a little bit sparingly, and sometimes with very small quantities of the carbonate and the chloride of sodium; then the mixture, covered with a layer of starch powder, is placed in an earthen crucible and subjected to a temperature of approximately 1000° C., which is maintained for 5 hours, after which it is allowed to cool slowly inside the furnace. The proportions requisite to obtain the optimum vary with the nature of metals tested; from the first I confirmed the superiority of bismuth, especially when the reaction was with strontium sulphide. By following my process and using only half a milligram per 100 grams

*Adaptation of an address given before the Congress at Valladolid of the Spanish Association for the Advancement of Sciences.

of carbonate, I obtained magnificent effects, of surprising duration; the phosphorescence, of a clear green color, is so persistent that after 18 years the sulphides retain the maximum of sensibility and the maximum of intensity of their phosphorescence, without perceptible loss.

But the bismuth is far from being the only metal endowed with qualities so remarkable; others possess them likewise in a high degree; I believe I was the first to study them in manganese. As early as 1886 Lecoq de Boisbaudran had noted certain fluorescent properties in manganese, but his researches were very different from mine of 1899 on manganese as an active substance in the phosphorescent sulphides of strontium, prepared by my method. It is of the greatest interest to me to recall this, since the calcium sulphides, wherein I first observed phototropy had been prepared with primary substances containing manganese.

Let me note another fact, often observed in my researches, and published as early as 1897, after having been thoroughly controlled. I sometimes extracted white masses from the crucibles and immediately introduced them into well-closed flasks in order to preserve them from the humidity and oxidizing action of the air. These bodies, though sensitive to the light, and impressionable to a certain degree, have not the intensity of phosphorescence that might be expected. But if, before placing them in the flasks, they are submitted to the action of the air for a few minutes, so that there is a beginning of oxidation, then the phosphorescence develops with the greatest intensity, and its duration is very long and permanent. The better to demonstrate this I preserved in inert atmospheres certain sulphides which were not phosphorescent nor even impressionable, but which became so with the greatest intensity as soon as they had been subjected to a commencement of oxidation; this, therefore is necessary for obtaining the best effects of photoluminescence. As soon as the sulphide has been subjected to the influence of light we note likewise, that the color changes, and it becomes reddish or violet; if the light has not been intense enough, or if it has been deprived of direct illumination, it returns to its primitive white or grayish color; but as soon as it is submitted to direct light, the change of color is reproduced, without the necessity of isolation. It is precisely this which is the fundamental fact of the phototropy of inorganic systems.

It may be obviously deduced, as a general law, that pure white sulphides are never phosphorescent, nor visibly sensitive to light. It is necessary that they be mixed with minute proportions of active substances (of the most varied nature), and then, after the sulphide mass has been thoroughly thus impregnated, raised to a very high temperature, maintained for several hours. This foreign substance, therefore, seems to be the determining cause of their phosphorescence. From this fact is derived the general manner of considering the structure of phosphorescent bodies, not as simple aggregates or mixtures of various unrelated substances, but, on the contrary, as complex masses in which we must distinguish two principal systems. We must consider, on the one hand, the sulphide, containing, or not containing, very small proportions of alkaline substances; and on the other hand the substances which serve to render it impure. These two systems seem to be in a certain measure independent; but they cannot remain indifferent with respect to each other for the phosphorescence depends, with regard to its color, duration, and intensity, upon the relative proportions of the heavy metal, within the limits assigned by the experimental law of the optimum.

There is no difficulty in admitting the co-existence of these two systems in the interior of every photoluminescent body, each with its own individuality, but in an intimate relation of dependence and mutual action in the production of the phenomenon. From this consideration we deduce that all the photoluminescent systems are true solid solutions, endowed with peculiar properties determined in the last analysis by the conjunction of the two systems which compose them. From a purely chemical point of view we know nothing certain as to their actions. However, we cannot deny that photoluminescence is fundamentally a photochemical phenomenon, in spite of our comparative ignorance of the mechanism of the molecular transformations realized between the two systems, when their *ensemble* after having received the direct impression of light, becomes phosphorescent in the dark, for a certain length of time. In appearance, nothing is changed; except the luminosity, nothing changes of chemical nature, indicated on the exterior, yet we are completely convinced that molecular changes have been effected, and we regard all manifestations of phosphorescence as molecular phenomena. We admit that the white alkaline-earth sulphide is inert with respect to light, and that by itself it cannot become phosphorescent. Hence it must be excited by another substance intimately united with it. Since none of the substances which we mix with the sulphides of barium, strontium, and calcium are themselves phosphorescent or sensitive to light in the free state, it must be admitted that such sensitiveness is necessarily an immediate function of the relations between the two systems. We call the metallic compound disseminated in the mass of the sulphide the *active* or *phosphorogenic* substance, and the sulphide itself the solvent or dilutant, terms which in themselves indicate the attribution of the principal part of the phenomenon to the infinitesimal portion of metallic substance.

This suggests that the phosphorogenic system acts in the manner of a catalyzer, whose functions called into action by luminous excitation, which provokes, possibly, molecular changes, and which, when it ceases to act, will give place to an inverse variation, with the production of phosphorescence.

According to this hypothesis the sulphide mass acts as a very peculiar solid solvent. I have already shown, by the phenomena which I have termed *auto-excitation*, that it is sufficient for any surface whatever of the mass of a photoluminescent body to be excited to cause the excitation to be propagated throughout it, even to portions which are not lighted; this propagation is by progressive zones. And I have also shown that sulphides excitable by light retain a very feeble residue of their alteration, which accumulates, and in time becomes the cause of increase of sensibility. To observe the first phenomenon it suffices to cover with black paper the flask containing these excitable sulphides, which have remained inert for a long time, making a narrow slit in the black cover to admit the light. If we then subject it to direct intense light for 3 minutes and remove the cover from the flask in the dark, we see that the whole mass shines with intense phosphorescence, though only a tiny portion has received direct light.

Let me add that in certain phosphorescent sulphides marked augmentations of their sensibility may be observed, and also that the phosphorescence, far from diminishing, visibly augments, even in case of the commencement of oxidation of the body. In my opinion, we have here the effect of residues of excitation of reiterated impressions. I preserve a magnificent example of calcium sulphide, very white and finely powdered, which becomes daily more excitable, and which emits light, not only in the dark, but in a place which is dimly lighted, for more than an hour, with violet phosphorescence of the highest intensity, after having been excited by exposure to direct light for a few seconds. I possess another, strontium sulphide, imbued with the most brilliant green phosphorescence, which has the same superior qualities. Each is 18 years old. In the calcium sulphide the active substance is bismuth, in the proportion of a half milligram per 100 grams of sulphide, while in the strontium sulphide is manganese in the same proportion, while each contains only 3 milligrams of sodium chloride, and 2 milligrams of sodium carbonate, distributed in the mass by impregnation.

The active substance of the phosphorogene, therefore, is not active of itself in an isolated state, and this fact relates it to certain catalyzers, of a photochemical nature, which do not enjoy the property of accelerating the speed of reactions, or of provoking them and causing them to continue, except when light exerts its action upon them in the presence of other bodies. If we accept this hypothesis we perceive the necessity for the whiteness and transparence of the sulphurized solvent since it is necessary for it to permit the passage of the luminous rays which are to exert their influence upon the infinitesimal particles of the active substance, which are distributed pretty uniformly in the mass of the system. But what we cannot explain without recourse to the effect of chemical reactions is why the alkaline-earth sulphides, or those of zinc, equally white, should always be the only possible solvents, and not some other white substance less alterable in air. Even less explicable is the necessity of forming the solid phosphorescent solutions at the same time that the solvent sulphides are engendered, at a very high temperature. It is this which makes me believe that the cause and the mechanism of this singular fact must be sought in the methods of formation of the different systems, and in the physico-chemical relations of the two components of the solid solution.

Since there are two distinct systems it is necessary to determine which is the one truly active, or whether the activity inheres in both, or in their conjunction. The accepted view is that the phosphorogene is the sensitive system, and that this upon being excited reacts upon the solvent. As soon as the body is carried into the dark the reversible action begins, and lasts until the re-establishment of the primitive state or of *equilibrium*, provoking intense phosphorescence during this time. But such a hypothesis prejudges nothing with regard to the molecular reactions which must necessarily exist between the components of the solid solution, nor upon the nature of the reversible transformations which are revealed by luminous phenomena.

Other observers—partisans of the electronic theory of phosphorescence—believe that both the system of the phosphorogene and that of the solvent are in a veritable critical state as regards luminosity, which the slightest excitation serves to "touch off." Thereupon the phosphorogene, which this theory terms the *electronogene*, emits negative electrons, which, projected upon the solvent, or *luminophore*, alter its equilibrium, render it luminous, in such sort that the rôle of the active substance is reduced to that of a kind of photogenic excitant. It is in the dark that we perceive changes so subtle, reduced, as it were, to a highly complex electronic manifestation. Perhaps such a hypothesis may explain other cases of phosphorescence (of which many varieties are known), especially cathodic phosphorescence; but I do not see how they can be applied to photoluminescence, which I have so long attempted to explain. What the electronic hypothesis does, is to pose the question in terms of greater clarity, by pretending to resolve it by establishing the relations of resemblance and the different functions of the phosphorogene, or active substance, and of the solvent, which is not considered as an inert and almost insensitive thing, as in other theories.

We must not forget the important fact that all the photoluminescent systems are dilute *solid* solutions, subject, possibly, to the laws of dilute *liquid* solutions. In this case we must suppose that the active body is ionized and dissociated in ions, and consider the solvent as the ionizing body; this is another hypothesis or rational foundation; but like the preceding, it seems established rather for the interesting phenomena of cathodic phosphorescence than for those, apparently more simple, of the photoluminescence of the white alkaline-earth sulphides. However, we can extract some advantages from this hypothesis, with regard to the latter. We know positively (law of the optimum) that there exists a determinable numeric relation between the intensity of the phenomenon, the sensitiveness to light, and the quantity of active metal contained in the system. From the viewpoint of the indicated hypothesis we might say, more correctly, that to a certain degree, numerically determinable, the sensibility of the system towards light, and consequently the intensity of its phosphorescence, depends upon the concentration of the active substance of the phosphorogene.

Strictly speaking, neither this angle of view, nor the new enunciation, teaches us anything as to the cause and mechanism of photoluminescence. The law of the optimum is a good experimental law, undoubtedly very useful, which indicates a limit; but it does not give a reason for the fact nor permit it to be foreseen. Even less does it explain the changes of color in the systems under the direct action of light, i. e., the cases of phototropy which often accompany phosphorescence, and even cathodic fluorescence. Moreover, the chemical aspect of the phenomenon remains unsolved. The hypothesis tells us nothing of the state of the phosphorogene in the solution, nor if it is in the form of the sulphide, or, as there is some reason to suppose, in the state of an intermediate oxide, passing instantly to the sulphide under the influence of light, and becoming the oxide again in the dark, while restoring the actual radiations which it had absorbed—which is the opinion of partisans of the purely chemical interpretation of photoluminescence.

It seems to me to conform better to the phenomena, as I have said, to regard the active matter as a photochemical catalyzer. We know how great the chemical activity of light is, and how considerable its influence as an accelerator of chemical reactions; this is why it is at times an extraordinary catalyzer, of which the photosyntheses, now so numerous, are conclusive proof. From the chemical point of view, both the sulphide and the metal are quite inert, just as are other substances which depend upon light to cause molecular transformations. We note too that in catalysis minute quantities of active elements are capable of causing great changes, and that their activity is not connected with their nature, but seems derived from exterior causes, such as temperature, state of mechanical division, and illumination. The two latter appertain to the phos-

phorogene, which may well be a catalyzer whose actions are arrested at the intermediary phase recognized in almost all catalytic actions, and which, by reason of its instable condition, renders the phenomenon reversible.

It was in 1907 that I observed, in calcium sulphide having bismuth for its activator, the change of color which characterizes phototropy, and I believe I was the first to note such a phenomenon. The color change in white sulphides under intense illumination is pretty general, and is not incompatible with photoluminescence; and is even seen in bodies having cathodic fluorescence.

However, the new phenomenon has a certain characteristic individuality, for I have prepared series of bodies which were very phototropic, and absolutely deprived of phosphorescence or fluorescence, always of white sulphides. I succeeded in separating, from a large collection of phosphorescent bodies, a first series of calcium sulphides, which as soon as subjected to strong direct daylight changed from white to pink and then to violet, immediately arriving at their maximum intensity. Taken into diffused light they returned to their primitive white color in a few moments, and were always ready for such changes, despite their age, an excellent example of reversible phototropy; some of these were both photoluminescent and phototropic. Some only phototropic, and some only phosphorescent. In those which changed color various metals acted as phosphorogene, bismuth, copper, uranium, thorium, zinc, manganese, antimony, etc.

By analysis of the phototropic systems of calcium sulphides I proved that without exception all exceedingly minute proportions of manganese as coming from the quick lime or calcium carbonate used as raw material. But it was not possible to detect it in other systems, which, while very sensitive to light, are not phototropic; and those to which a trace of manganese was specially added as a phosphorogene acquired both properties. I concluded that manganese is not only a remarkable phosphorogene, but a splendid phototrope. By repeated experiments I proved the essential influence of manganese in the changing of color by light.

Having arrived at this first result, so very important, I undertook to examine the problem under other aspects. In the first place, is phototropy an exclusive property of manganesic calcium sulphide, or do other white sulphides, or do other sulphides, especially that of strontium, exhibit it? Next, what are the most efficacious proportions of the phototrope and its limits? It was also interesting to know if any active substance besides manganese could affect the phototropic reactions of this metal, and to what degree. To solve these problems I undertook experiments with strontium sulphide and calcium sulphide separately, but in the same proportions, first with manganese and then with bismuth, in the form of the subnitrate or chloride.

Lack of space prevents us from following the author through the details of these later experiments, one interesting result of which was to prove that manganese and bismuth used as active matters in conjunction had a combined effectiveness in producing intensity of phototropy. Prof. Mourelo closes his valuable and stimulating account of his researches with the announcement of the following law: The sensibility of the systems of the sulphides of strontium and calcium to light, and likewise their phototropy, depends in a certain measure on the concentration of the active substance.

Germany's Manganese Supply

Failure of all Substitutes for Ferromanganese—Use of Calcium Carbide

By Frank Jovic

WITH reference to the very interesting analysis of the ferromanganese question in Germany, published in *The Iron Age*, Dec. 28, 1916, the following statement of facts may help to clear the situation:

Owing to the complications in the manganese ore trade previous to the war, statistics are not clear and give no reliable information. Official data stop with 1913 and the heavy shipments of the first half of 1914 are not recorded. The fact that most of the ore was unloaded either in Antwerp or in Rotterdam makes all figures unreliable, because from those ports they were sent either to Belgium, French Lorraine, German Lorraine, Luxemburg or Westphalia, both by boat and by rail, passing and often repassing the different border lines.

One thing is indisputable, Germany is very short of manganese, and all substitutes have failed to give as satisfactory results in spite of the assertions in the daily press. This is emphasized by the commandeered silence on the question of steel quality and the high price the German iron works are willing to pay for high-grade manganese ore. The substitutes that were tried out and disappointed many chemists were more costly and yielded such poor results that ferromanganese percentage is still the most used recarburizer of steel.

The greatest hopes were based on calcium carbide, CaC_2. Everybody knows this stone-like solid, which is used extensively in the manufacture of acetylene gas. From the chemical standpoint all the properties of this compound indicated a substitute for the ferromanganese. In fact, CaC_2 is inflammable, infusible and non-explosive, it can be manufactured in any quantities out of limestone and coal, and is sold at the very reasonable price of $70 per ton. Made in the electric furnace with considerable absorption of heat, it is one of the most energetic reducing agents known; it dissociates even water, yielding C_2H_2 and CaO. It contains carbon, a hardener.

Thus it seems that both elements of calcium carbide could be active in the deoxidation and recarburization period in the manufacture of steel. The calcium splits up iron oxide into its elements, leaving the iron and forming CaO, a slag easily rising out of the bath. The carbon, which alloys with the iron, gives the right composition. Furthermore, being generated endothermically, CaC_2 gives heat to the bath in dissociating—a favorable effect, making the bath more fluid, helping the reaction, the expelling of gas and separation of CaO.

The use of calcium carbide has been recognized at regular intervals by inventors, even at times when its price was higher than that of ferromanganese. I refer to United States patents No. 868,610 and No. 1,081,532, and others as far back as 1907.

In spite of all these apparently favorable conditions, CaC_2 failed to give results in persistent tests, made on a large scale during many months. The main reason is the action of the calcium on iron. Calcium is all right for deoxidation, but inert to iron; it is indifferent. Manganese, on the contrary, besides its deoxidizing effect on the molten bath, alloys with the iron, hardens the metal, improves its quality and thus promotes the easy operation so universally used to-day. Furthermore, that metal is easily marketed in all grades, ranging from 10 to 80 per cent manganese, to satisfy any condition.

Greater success seems to be secured by the use of thermit-aluminum products, advertised previous to the war, although their use necessitates caution and is far from general.

It does not seem probable that the end of the war will surprise the world with great discoveries in the line of substitutes. The price of above $150 a ton for ferromanganese makes anywhere a promising substitute very profitable. The war only emphasizes one thing—that the products on which the iron industry previous to the war were based were sound and thoroughly tested by the sharp world-wide competition of the long peace period previous to the war. Then, too, chemists and engineers were busy to lower the manufacturing cost and promote efficiency. Laboratories as well as universities were eagerly at work to investigate all possible chances of new materials and products.

Fortunately or unfortunately, as one will have it, these raw products do not recognize in their distribution the principle of the nationalities. —*The Iron Age.*

An Asbestos Packing

KLINGERITE is the trade name of a material of German manufacture, used as a packing for steam pipe joints, for which purpose it is excellently adapted. The shortage due to the war has been extensively felt and attempts —not completely successful—have been made in France to produce a substitute. The author publishes the results of an examination—chemical and physical—of the material, with practical suggestions as to how it might be prepared in a satisfactory condition. A sample in the form of a card 3.75 mm. thick was found to consist of fifteen thicknesses of a thin felt of asbestos fiber mixed with about 2 per cent of flax threads. The asbestos was of the finest commercial quality of long fiber. Each layer appears to have been coated on both sides with an agglutinant composed of a mixture of rubber and balata with a small quantity of sulphur. By pressure the whole had been worked into a uniform and compact mass. When the packing is heated in use vulcanization occurs, rendering it more impervious and accordingly more resistant to the destructive action of steam under pressure. The results of chemical analysis gave:—Asbestos, 80.46 per cent; cellulose (flax), 2.24 per cent; agglutinant, 17.16 per cent (rubber, 5.19, balata, 11.97 per cent); sulphur, 0.13 per cent. The percentage of sulphur is doubtful; the author suggests the use of 5 per cent of the weight of agglutinant. The purpose of the flax is apparently to strengthen the constituent layers of asbestos for the process of coating with the agglutinant.—*A. Dubois in Caoutchouc et Gutta-Percha. From a note in the Journal of Society of Chemical Industry.*

Treatment of Ivy Poisoning

STUDIES of rhus toxicodendron show that its poisonous principle is a volatile acid resin; therefore, the treatment of rhus poisoning with an alkali is theoretically right. In practice the correctness of the theory is borne out; a saturated aqueous solution of sodium bicarbonate being one of the most efficacious remedies. Recently a note was made in these columns of the application of ice water for rhus poisoning in the United States Army with good results. The possibility of combining the alkaline and ice water treatments naturally suggests itself, and upon trial this has been found to be prompt and efficacious. In two cases which have come to our notice a saturated solution of sodium bicarbonate in ice water applied freely and continuously to the infected area brought about prompt relief and a complete cure within twelve hours. In view of the suffering entailed by a severe attack of rhus poisoning and of the failure of the older method of the treatment with opium and lead water, grindelia, etc., it is well to bear in mind this method, which is easy of application and prompt in affording relief.—*New York Medical Journal.*

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Table of Contents

PAGE

Relativity and Gravitation.—By James Rice . . . 210
Cast Iron Growth . . . 211
Diesel Engines . . . 211
The Baghdad Railway.—3 Illustrations . . . 212
A Prairie Dog Feud . . . 213
The Metric System.—By George F. Kunz . . . 213
Inferences Concerning Auroras . . . 214
The Fixation of Nitrogen as Cyanide . . . 214
An Industrious Elephant . . . 215
An Eruption of Lassen Peak.—By Andrew H. Palmer. 7 Illustrations . . . 216
An Explosion Effect . . . 217
Progressive Oxidation of Cold Storage Butter.—By D. C. Dyer . . . 217
Soap Bubbles of Long Duration . . . 218
Detection of Artificial Coloring Matters in Wine . . . 218
The Value of High-Level Meteorological Data II.—By S. P. Fergusson.—3 Charts . . . 219
Phosphorescence.—By Jose R. Mourelo . . . 220
Germany's Manganese Supply . . . 221
Asbestos Packing . . . 221
Treatment of Ivy Poisoning . . . 222